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Philosophy of Science

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Philosophy of Science

VOL. I January, 1934 NO. I

EDITORIAL

What is Philosophy of Science?

PHILOSOPHY OF SCIENCE is the organized expression of a growing intent among philosophers and scientists to clarify, perhaps unify, the programs, methods and results of the disciplines of philosophy and of science. The examination of fundamental concepts and presuppositions in the light of the positive results of science, systematic doubt of the positive results, and a thorough-going analysis and critique of logic and of language, are typical projects for this joint effort. It is not necessary to be committed to a belief that science and philosophy are or should be one, or else that "never the twain shall meet." If anything, there is to be expected a wholesome regard for the value of established science in furnishing a foil for philosophy and a check on its old extravagances. This does not mean that even the best-established science may not be subject to most devastating criticism by analysis of its foundations. In fact, despairing of the philosophy of the schools, science has done it largely for itself. The theories of gravitation, atomicity, electro-magnetism, evolution, relativity and quanta have all arisen through drastic revisions of complacent fundamental "truths."

Editorial

Brief as the title of our journal may be it alludes to two worlds which oft have been ill-defined and misconceived, and indeed offers opportunities for further distortion in the conjunction. Yet, optimistically considered, therein may be a promise of greater riches on clarification. We quote a few contrasts of opinion:

"The Philosophy of Science is an application of the scientific method to philosophy. . . . Science always implies Monism, that is, a unitary world-conception."—*The Monist.*

"Thus, Chapter II of my *Studies*, which is entitled 'The Idol of Scientific Method,' aims at showing that, if philosophy is to render in coherent theory the real nature of the universe as revealed in our experience, it cannot model its procedure on that of the sciences."—*R. F. Alfred Hoernle.*

"A fully 'explained' world would be indistinguishable from pure non-being."—*A. E. Taylor.*

"The future of Philosophy is the elevation of Science to the level of a philosophy. . . . Science regards man as an *effect* of nature, philosophy as a *cause* in nature (knowing the laws of nature)."—*Aloys Riehl.*

"Nature is closed to Mind."—*Alfred North Whitehead.*

"The Earth is a globe, and what it is besides may be found in the museums of natural history."—*Gustav Theodor Fechner.*

"Don't forget at any moment that thou art a *human being* and not a mere product of nature."—*David Friedrich Strauss (1808–1874)—a Hegelian.*

"Without science, philosophy is null; without philosophy, science is blind."—*Cournot.*

"Theory is not merely an economic presentation of laws, but a classification at the same time of these laws."—*Pierre Duhem.*

"No science can safely be abandoned entirely to its own devotees."—*Venn.*

"Every plank [of the advance of science] is first laid by Retroduction alone, that is to say by the spontaneous conjectures of instinctive reason."—*C. S. Peirce.*

"It is one of the most important functions of science to achieve as *perfect* an elaboration as possible of *all* the relations conceivable, and in this practical necessity lies the foundation of the general or theoretical elaboration of science."—*Wilhelm Ostwald.*

"The field of the theoretical philosophy of nature,—yes, the field

of the logic of science—this whole region is to-day an open one."—*Josiah Royce.*

"Metaphysics is the queen of the sciences; but this queen has been for a long time an over-delicate and even a sickly woman. She is in great need of a propping-up from her healthy and strongly developed sisters—the natural sciences."—*Erich Becher.*

"Metaphysics will probably be abandoned altogether. . . ."—*Charles Richter.*

"Space is only a word that we have believed is a thing. What is the origin of this word and of other words also? What things do they hide? To ask this is permissible; to forbid it would be, on the contrary, to be a dupe of words; it would be to adore a metaphysical idol, like savage peoples who prostrate themselves before a statue of wood without daring to take a look at what is within."—*Henri Poincaré.*

* * *

In our group we have representatives of practically all the shades of opinion suggested by the quotations—radicals, progressives, a few tried veterans of established philosophic fashion, but no reactionaries,—a coalition dominated by the unorthodox.

* * *

In style we allow the greatest latitude. The editorial remarks could have been made very solemn,—but that is really too easy.

In content we require accuracy, thoroughness, rigor—in the fashion of a research journal.

* * *

We propose to elaborate a program for the Philosophy of Science, the subject and the journal,—a research program, and like all good research programs subject to revision with progress. Some kind of priority list is necessary. The editor submits a general listing, solely on his own responsibility:

I. Studies in the analysis of meaning, definition, symbolism.
II. Studies in presuppositions—axioms, postulates, maxims.
III. Studies in method.
IV. Studies of the nature and formulation of theoretical principles.

V. Studies in the structure of the sciences, their hierarchies.
VI. Studies in the function and significance of science within various contexts.

* * *

For the time being our departments will be

EDITORIAL
CONTRIBUTIONS
DISCUSSION (CORRESPONDENCE)
REVIEWS AND NOTES
BUDGET OF PARADOXES
NOTICES

Their purposes will be apparent from their content and from notes appended under the headings.

W. M. M.

On the Character of Philosophic Problems[1]

BY

RUDOLF CARNAP

PHILOSOPHY IS THE LOGIC OF SCIENCE

PHILOSOPHERS have ever declared that their problems lie at a different level from the problems of the empirical sciences. Perhaps one may agree with this assertion; the question is, however, where should one seek this level. The metaphysicians wish to seek their object *behind* the objects of empirical science; they wish to enquire after the essence, the ultimate cause of things. But the logical analysis of the pretended propositions of metaphysics has shown that they are not propositions at all, but empty word arrays, which on account of notional and emotional connections arouse the false appearance of being propositions. This conception that the "propositions" of metaphysics, including those of ethics, have no theoretical content, is to be sure still disputed. We shall not, however, enter here on its demonstration, but, under its guidance, will limit ourselves to non-metaphysical and non-ethical (non-evaluating) philosophical problems.

[1] Translated by W. M. Malisoff. Attention is called to the following choices taken by the translator:—*Auffassung* has been rendered variously as interpretation, conception, position; *Folgerung* as deduction, conclusion, inference, but in conformance with the discussion, most often as entailment. *Gehalt* which may mean value, has been rendered only as *content; Inhalt* as meaning; but *inhaltlich* as *connotative*, rather than *strict* or *meaningful* or *intensional*, which may convey as much.

In order to discover the correct standpoint of the philosopher, which differs from that of the empirical investigator, we must not penetrate *behind* the objects of empirical science into presumably some kind of transcendent level; on the contrary we must take a *step back* and *take science itself as the object. Philosophy is the theory of science* (wherein here and in the following "science" is always meant in the comprehensive sense of the collective system of the knowledge of any kind of entity; physical and psychic, natural and social entities). This must be appraised more closely. One may consider science from various viewpoints; e.g. whether one can institute a psychological investigation considering the activities of observation, deduction, formulation of theories, etc., or sociological investigations concerning the economical and cultural conditions of the pursuit of science. These provinces—although most important—are not meant here. Psychology and sociology are empirical sciences; they do not belong to philosophy even though they are often pursued by the same person, and have torn loose from philosophy as independent branches of science only in our own times. Philosophy deals with science only from the *logical* viewpoint. *Philosophy is the logic of science*, i.e., the logical analysis of the concepts, propositions, proofs, theories of science, as well as of those which we select in available science as common to the possible methods of constructing concepts, proofs, hypotheses, theories. [What one used to call epistemology or theory of knowledge is a mixture of applied logic and psychology (and at times even metaphysics); insofar as this theory is logic it is included in what we call logic of science; insofar, however, as it is psychology, it does not belong to philosophy, but to empirical science.]

The interpretation that philosophy is the logic of science is not to be justified here. It has been represented previously and is represented now by various philosophic groups, amongst others also by our Vienna circle. With this thesis the question as to the character of philosophic problems is not by any means already solved. Very much comes into question right at this point. We should consequently ask here: what character, what logical

nature, do the questions and answers of the logic of science have? For those who are with us in the conception that philosophy is the logic of science the question of the character of philosophic problems will be answered thereby as well.

ARE THE PROPOSITIONS OF THE LOGIC OF SCIENCE MEANINGLESS?

Our antimetaphysical position has been formulated by Hume in the classical manner:—

"It seems to me, that the only objects of the abstract science or of demonstration are quantity and number, and that all attempts to extend this more perfect species of knowledge beyond these bounds are mere sophistry and illusion. As the component parts of quantity and number are entirely similar, their relations become intricate and involved; and nothing can be more curious, as well as useful, than to trace, by a variety of mediums their equality or inequality, through their different appearances. But as all other ideas are clearly distinct and different from each other, we can never advance farther, by our utmost scrutiny, than to observe this diversity, and, by an obvious reflection, pronounce one thing not to be another. Or if there be any difficulty in these decisions, it proceeds entirely from the undeterminate meaning of words, which is corrected by juster definitions."—Hume, An Enquiry Concerning Human Understanding, XII, 3.

Against this the following objection, which on first appearance seems indeed destructive, has been repeatedly raised:—"If every proposition which does not belong either to mathematics or to the empirical investigation of facts, is meaningless, how does it fare then with your own propositions? You positivists and antimetaphysicians yourselves cut off the branch on which you sit." This objection indeed touches upon a decisive point. It should be of interest to every philosopher as well as metaphysician to comprehend the character of the propositions of the logic of science; but to the antimetaphysician, who identifies philosophy and the logic of science, this is the *deciding question*, upon the satisfactory answer of which the security of his standpoint depends.

Wittgenstein has represented with especial emphasis the thesis of the meaninglessness of metaphysical propositions and of the

identity of philosophy and the logic of science; especially through him has the Vienna circle been developed on this point. How now does Wittgenstein dispose of the objection that his own propositions are also meaningless? He doesn't at all; he agrees with it! He is of the opinion that the non-metaphysical *philosophy* also has no propositions; it operates with words, *the meaninglessness of which in the end it itself must recognize:—*

"Philosophy is not a theory but an activity. A philosophical work consists essentially of elucidations. The result of philosophy is not a number of "philosophical propositions," but to make propositions clear." (p. 77)

"My propositions are elucidatory in this way: he who understands me finally recognizes them as senseless, when he has climbed out through them, on them, over them. (He must so to speak throw away the ladder, after he has climbed up on it.) He must surmount these propositions; then he sees the world rightly. Whereof one cannot speak, thereof one must be silent." (p. 189, Tractatus Logico-philosophicus)

We shall try in the following to give in place of this radically negative answer a *positive answer* to the question of the character of the propositions of the logic of science and thereby of philosophy.

CONNOTATIVE AND FORMAL CONSIDERATION

(Inhaltliche und formale Betrachtung)

To construct science means to construct a system of propositions which stand in certain fundamental coherence with one another. The logic of science is thus the logical analysis of this system, of its elements and of the methods of tying these elements. In such an analysis we can start from but two different viewpoints; we shall call them connotative (inhaltlich) and formal.

It is usual in the logic of science to put something like the following and similar questions: What is the meaning of this or that concept? In what relation does the meaning of this concept stand with respect to that? Is the meaning of this concept more fundamental than of that? What meaning (Inhalt, Gehalt) does this proposition have? (Or: What does this

proposition say?) Is the meaning of this proposition contained in the meaning of that? Does this proposition say more than that? Is what this proposition asserts, necessary or contingent or impossible? Is what these two propositions say compatible?

All these questions refer to the *meaning* of concepts and propositions. We call them therefore questions of meaning or of *connotation* (inhaltliche). In contrast to this we understand by *formal* questions and propositions such as relate only to the formal structure of the propositions, i.e. to the arrangement and kind of symbols (e.g. words) out of which a proposition is constructed, *without reference to the meaning* of the symbols and propositions. Formal (in the sense here defined) are e.g. (most of) the rules of grammar.

According to prevalent conception the connotative questions of the logic of science are much richer and fruitful than the formal; though the formal do belong to the logic of science, they are at most a small, insignificant section. But this opinion is wrong. The logic of science can progress without exception according to the formal method without thereby restricting the wealth of questioning. It is possible in case of purely formal procedure, that is from a viewpoint in which one does not reckon with the meaning, finally to arrive to the answering of all those questions which are formulated as connotative questions. This possibility is to be shown illustratively in the following. Therewith the question of the character of *philosophy as logic of science* is answered: it is *the formal structure theory of the language of science*,—we shall call it: The logical syntax of the language of science.

LOGICAL SYNTAX OF LANGUAGE

By the "logical syntax" (or also briefly "syntax") of a language we shall understand the *system* of the *formal* (i.e. not referring to meaning) *rules* of that language, as well as to the consequences of these rules. Therein we deal first with the *formative rules* (Formregeln) which decree how from the symbols (e.g. words) of the language propositions can be built up, secondly with the *transformation rules* (Unformungsregeln), which decree how from

given propositions new ones can be derived. If the rules are set up strictly formally they furnish mechanical operations with the symbols of the language. The formation and transformation of propositions resembles chess: like chess figures words are here combined and manipulated according to definite rules. But thereby we do not say that language is nothing but a game of figures; it is not denied that the words and propositions have a meaning; one merely averts methodically from meaning. One may express it also thus: *language* is treated as a *calculus*.

That the formal, calculus-like representation of the formative rules is possible is evident. What linguists call rules of syntax are indeed such formal (or at least formally expressible) rules for the formation of propositions. We can see, however, clearly that the transformation rules, which one usually calls logical rules of deduction, have the same formal, that is, syntactical character. (And that is the reason why we call the combined system of rules syntax, in widening the terminology of linguists). Since Aristotle the efforts of logicians (more or less consciously) were directed toward formulating the deductive rules as formally as possible, i.e. possibly so that with their help the conclusion could be "calculated" mechanically from the premisses. This was attained first in a strict manner only in modern symbolic logic; the traditional logic was too much hindered by the defect of the language of words.

For a certain part of the language of science we already know a strictly formal theory, namely *Hilbert's* mathematics. It considers the symbols and formulas of mathematics without reference to meaning, in order to investigate relations of deducibility, sufficiency, consistency, etc. This mathematics is hence (in our manner of expression) the logical syntax of mathematical language. The logical syntax of the language of science meant here is an analogous extension with reference to the language of all of science.

One of the most important concepts of logic and thereby of the logic of science is that of (logical) inference (Folgerung-entailment). Can this concept be formulated purely formally? It is often stated that the relation of entailment depends on the

meaning of the propositions. In a certain sense we can agree with that; for when the meaning of two propositions is known, it is thereby determined whether one is the entailment of the other or not. The decisive point, however, is: is it also possible to formulate the concept "entailment" purely formally? If the transformation rules of language are set up purely formally, we call a proposition an inference (entailment) of other propositions if it can be constructed from those propositions by the application of the transformation rules. The question, whether a certain proposition is an inference (entailment) of certain other propositions or not, is therefore completely analogous to the question whether a certain position in chess can be played from another or not. This question is answered by chess theory, i.e. a combinatorial or mathematical investigation which is based on the chess rules; that question is thus a formal one, it is answered by a *Combinatorial Calculus or Mathematics of Language*, which rests on the transformation rules of language, that is what we have called the *syntax* of language. Briefly: "entailment" is defined as deducibility according to the transformation rules; since these rules are formal, "entailment" is also a formal, syntactical concept.

The concept "entailment" is, as *Lewis* has correctly seen, quite different from the concept of "(material) *implication*." (Russell, Principles of Mathematics). Implication does not depend on the sense of the propositions, but only on their *truth-value;* but entailment on the contrary is not quite determined by the truth values. From this, however, one may not conclude that in the determination of entailment reference to the *meaning* is necessary; it suffices to refer to the formal structure of the propositions.

THE CONTENT OF A PROPOSITION

On the basis of the concept "entailment" one can define the following classification of propositions which is fundamental to the logic of science. A proposition is called *analytic* (or tautological) if it is an entailment of every proposition (more exactly: if it is deducible without premisses, or is the entailment

of the empty class of propositions). A proposition is called *contradictory* if any proposition at all is its entailment. A proposition is called *synthetic* if it is neither analytical nor contradictory. Example: "It is raining here" is synthetic; "It is raining or it is not raining" is analytic; "It is raining and it is not raining" is contradictory. An analytic proposition is true in every possible case and therefore does not state which case is on hand. A contradictory propostion on the contrary says too much, it is not true in any possible case. A synthetic proposition is true only in certain cases, and states therefore that one of these cases is being considered,—all (true or false) statements of fact are synthetic. The concepts "analytic," "contradictory," "synthetic" can be defined in analogous manner also for classes of propositions; several propositions are said to be *incompatible* (unverträglich) with one another, if their class is contradictory.

And now we come to the principal concept of the logic of science, the concept of the (Inhalt) content of a proposition. Can this central concept of the connotative (inhaltliche) method of consideration be formulated purely formally also? We can be easily convinced that that is possible. For what, to be sure, do we want to know when we ask concerning the content or meaning of a proposition S? We wish to know what S conveys to us; what we experience through S; what we can take out of S. In other words: we ask what we can deduce from S; more accurately: what propositions are entailments of S which are not already entailments of any proposition at all, and therefore declare nothing. We define therefore: by the *content* (Gehalt) of a proposition S we understand the class of entailments from S which are not analytic. Thereby the concept "Gehalt" is connected to the syntactical concepts defined earlier; it is then also a syntactic, a purely formal concept. From this definition it is apparent that the content of an analytic proposition is empty, since no non-analytic proposition is an entailment of it. Further, that the content of S_2 is obtained from that of S_1 when and only when S_2 is an entailment of S_1; that two propositions are of equal content when and only when each is the entailment of the other. Thus the defined concept "Content" corresponds

completely to what we mean when we (in a vague manner) are accustomed to speak of the "meaning" (Inhalt) of a proposition; at any rate, insofar as by "meaning" something logical is meant. Often in the investigation of the "meaning" or "sense" of a proposition one also means: What does one think of or imagine in this proposition? This, however, is a psychological question with which we have nothing to do in a logical investigation.

CONNOTATIVE AND FORMAL MODES OF EXPRESSION

(Inhaltliche und formale Redeweise)

We have set out from the fact that a language can be considered in two different ways: in a connotative and in a formal manner. Now, however, we have established that with the aid of the formal method the questions of the connotative approach can also be answered finally. Fundamentally really there is no difference between the two approaches, but only a difference between two modes of expression: in the investigation of a language, its concepts and propositions and the relations between them, one can employ either the connotative or the formal mode of expression. The connotative mode of expression is more customary and obvious; but one must use it with great care, it frequently begets muddles and pseudo-problems. We shall consider several examples of propositions in connotative form and their translation into formal mode of speech; in the case of several of these examples (6a–10a) only on translation do we see that we are dealing with assertions concerning the language.

Connotative Mode of Speech:	*Formal Mode of Speech*
1a. The propositions of arithmetical language give the properties of numbers and relations between them.	1b. The propositions of arithmetical language are constructed in such and such a manner from predicates of one or more values and number expressions as arguments.
2a. The expression '5' and '3+2' mean the same number.	2b. 3b. The expressions '5' and '3+2' are synonymous in the arithmetical language (i.e. always interchangeable with one another).
3a. '5' and '3+2' do not mean the same number but two equal numbers.	

On the basis of the connotative formulation *1a* there arise easily a number of metaphysical pseudo-problems concerning

the nature of numbers, whether the numbers are real or ideal, whether they are extra- or intramental and the like. The danger of these pseudo-problems disappears when we use the formal mode of expression, where we speak of "number expressions" instead of "numbers." Also the philosophic conflict between *2a* and *3a* disappears in the formal mode of expression: both theses have the same translation.

4a. The word "luna" of the Latin language signifies the moon.	4b. On the basis of the syntactical translation rules between the Latin and the English languages the word "moon" is coördinated with the word "luna."
5a. The concept "red" signifies an ultimate quality; the concept "man" has a more ultimate meaning than the concept "grandson."	5b. The word "red" is an undefined fundamental symbol of language; the word "man" stands on a lower level that the word "grandson" in the definition family-tree of concepts.
6a. The moon is a thing; the sum of 3 and 2 is not a thing but a number.	6b. "Moon" is the designation of a thing; "3+2" is not a designation of a thing but a designation of a number.
7a. A property is not a thing.	7b. A property-word is not a thing-word.
8a. This particular (fact, event, condition) is logically necessary: .... logically impossible; .... logically possible.	8b. This proposition is analytic; .... contradictory; .... not contradictory.
9a. This particular (fact, event, condition) is physically necessary; .... physically impossible; .... physically possible.	9b. This proposition is deducible from the class of physical laws; .... is incompatible with ....; .... is compatible. ....
10a. Reality consists of facts, not of things.	10b. Science is a system of propositions, not of names.

PHILOSOPHY IS THE SYNTAX OF THE LANGUAGE OF SCIENCE

We had started with the presupposition: Philosophy of Science is the logic of science, the logical analysis of concepts, propositions, structures of propositions of science. Since now the data of every logical analysis can be translated in the formal mode of expression, all the questions and theorems of philosophy consequently find their place in the formal structure theory of language, that is, in the realm which we have called the Syntax of the language of Science. Here it must, however, be noted that a philosophic theorem, formulated as a proposition of syntax, can be meant in different ways:

A. As *Assertion;* e.g.
   1. In the language of science available today (or a part of it: of physics, biology, . . .) such and such holds.
   2. In every language (or: in every language of such and such a nature) such and such holds.
   3. There is a language for which such and such holds.

B. As *Proposal;* e.g.
   1. I propose to build up the language of science (or of mathematics, of psychology, . . .) so that it acquires such and such properties.
   2. I wish (along with other things) to investigate a language which possesses such and such properties.

The common confusion in philosophic discussions, not only among metaphysicians but also in the philosophy of science, is principally called forth by lack of a clear conception that the object of discussion is the language of science; and further because one does not clearly state (and mostly does not know oneself) whether a thesis is meant as an assertion or as a proposal. Let us consider, for example, in the discussion of the logical foundations of mathematics a point of conflict between the logisticists (Frege, Russell) and the axiomatists (Peano, Hilbert); let the theses be formulated by 12a, 13a. Then we translate the theses in order to formulate them more exactly into the formal mode of expression: 12b, 13b.

12a. The *numbers* are classes of classes of things.
13a. The numbers are unique ultimate entities.

12b. The number-symbols are class symbols of second rank.
13b. The number-symbols are individual-symbols (i.e. symbols of null rank, which appear only as arguments).

If now we interpret 12b and 13b in the manner A3, the conflict disappears: one can say that a language of arithmetic is constructible which has the property 12b; but also one as well which has the property 13b. But perhaps the theses 12b, 13b are meant as proposals in the sense B_1. In that case one is not dealing with a discussion about true or false, but with a discussion as to whether this or that mode of expression is simpler or more pertinent (for certain purposes of a scientific methodical nature).

In any case the discussion is oblique and fruitless as long as the discussers do not agree as to which of the interpretation A or B is meant. The situation is similar with regard to the philosophical combat concerning the theses 14a, 15a:

14a. To the ultimate given belong relations.

14b. To the undefined fundamental signs belong two- (or more-) valued predicates.

15a. Relations are never given ultimately but depend always on the nature of the members of the relation.

15b. All two- or more-valued predicates are defined on the basis of one-valued predicates.

The discussion becomes clear only when 14b and 15b are considered as proposals; the problem then consists of putting up languages of this or that form and to compare them to one another.

In the following example we deal with the conflict of two theses 16a, 17a, which correspond more or less to positivism and to realism.

16a. *A thing* is a complex of sensations.

16b. Every proposition in which a thing-name occurs, is of equal content with a class of propositions in which no thing-names but sensation-names occur.

17a. A thing is a complex of atoms.

17b. Every proposition in which a thing-name occurs is of equal content with a proposition in which no thing names but space-time coördinates and physical functions occur.

16b, 17b can be interpreted here in the sense A_1, namely as assertions concerning the syntactical structure of our language of science. In spite of that they do not contradict one another; since a proposition concerning a thing can be transformed in more than one way with equal content. We see: in using the formal mode of expression the pseudo-problem "What is a thing?" disappears, and therewith the opposition between the positivist and the realist answer disappears.

If we represent the position that all philosophical problems are questions of the syntax of the language of science, we do not mean it to be a proposal or even a prescription for limitation

to a definite, seemingly very narrow field of questions. Much more is meant: as soon as one exactly formulates some question of philosophy as logic of science, one notes that it is a question of the logical analysis of the language of science; and further investigation then teaches that each such question allows itself to be formulated as a formal question, to wit a question of the syntax of the language of science. All theorems of philosophy take on an exact, discussable form only when we formulate them as assertions or proposals of the syntax of the language of science.

THE PROBLEM OF THE FOUNDATIONS OF THE SCIENCES

In order to make clearer our position concerning the character of philosophic problems, we shall cast a brief glance on the problems which one customarily designates as the philosophic foundation problems of the individual sciences.

The philosophic *problems of the foundations of mathematics* are the questions of the syntax of mathematical language, and to be sure not as of an isolated language, but as of a part language of the language of science. This addendum is important. The logistic trend (Frege, Russell) is right in the demand that the foundation laying of mathematics must not only construct the mathematical calculus but also must make clear the meaning of mathematical concepts, since the application of mathematics to reality rests on this meaning. We restate it in formal mode of speech: mathematical concepts attain their meaning by the fact that the rules of their application in empirical science are given. If we investigate not only the syntactical rules of mathematical language merely, but also the rules which relate to the appearance of mathematical symbols in synthetic propositions, we formulate thereby the meaning of mathematical concepts (e.g. the meaning of the symbol "2" is formulated by establishing how this symbol can appear in synthetic propositions, and according to what rules such propositions can be derived from propositions without number expressions. If a rule is set up with the aid of which one can derive from the proposition "In this room there are Peter and Paul and otherwise no person" the proposition "In this room there are 2 people," the meaning of "2" is established by that rule).

The *problems of the foundations of physics* are questions of the syntax of physical language: the problem of the verification of physical laws is the question concerning the syntactic deductive coherence between the physical laws (i.e. general propositions of a certain form) and the protocol propositions (singular propositions of a certain form); the problem of induction is the question whether and which transformation rules lead from protocol propositions to laws; the problem of the finitude or infinity and other structure properties of time and space is the question concerning the syntactical transformation rules with reference to number expressions which appear in the physical propositions as time and space coordinates; the problem of causalty is the question concerning the syntactical structure of the physical laws (whether unique or probability functions) and concerning a certain property of completeness of the system of these laws (determinism-indeterminism).

The philosophical *problems of the foundation of biology* refer above all to the relation between biology and physics. Here the following two problems are to be distinguished:

1. Can the concepts of biology be defined on the basis of the concepts of physics? (If yes, the language of biology is a part language of physical language).
2. Can the laws of biology be derived from the laws of the physics of the inorganic? The second question forms the kernel of the vitalism-problem, if we purge this problem from the usual metaphysical admixtures.

Among the *problems of the foundations of psychology* there are analogously to the above-mentioned: 1. Can the concepts of psychology be defined on the basis of the concepts of physics? 2. Can the laws of psychology be derived from those of physics? The so-called psycho-physical problem is usually formulated as a problem of the relation of two object-realms: the realm of psychic events and the realm of physical events. But this formulation leads to a maze of pseudo-problems. In using the formal mode of expression it becomes clear that one is dealing only with the relation of both part-languages, that of psychology

and that of physics, and to be sure more accurately with the manner of the syntactical derivation relations (translation rules) between the propositions of both these languages. With the formulation of the psycho-physical problem in the formal mode of expression the problem surely is not yet solved; it may still be quite difficult to find the solution. But at least the necessary condition is satisfied whereby a solution may be sought: the question at least is put clearly.

A point of principle must now be noted so that our position will be understood correctly. When we say that philosophical questions are questions of the syntax of the language of science which permit expression in a formal mode of speech, we do not say thereby that the answers to these questions can be found by a mere calculating about with logical formulas without recourse to experience. A proposal for the syntactical formulation of the language of science is, when seen as a principle, a proposal for a freely chosable convention; but what induces us to prefer certain forms of language to others is the recourse to the empirical material which scientific investigation furnishes. (It is e.g. a question of convention whether one takes as the fundamental laws of physics deterministic or statistical laws; but only by attention to the empirical material, syntactically put: to the protocol propositions, can we decide with which of these two forms we can arrive at a well correlated, relatively simple construction of a system.) From this it follows that the task of the philosophy of science can be pursued only in a close cooperation between logicians and empirical investigators.

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The Logical Character of the Principle of Induction[1]

BY

HERBERT FEIGL

THE purpose of this paper is to make clear (1) that the widely recognized formulations of the principle of induction do not express the most fundamental rule of induction; (2) that the current view concerning the probability of induction must be revised in terms of a frequency theory of probability; (3) that on this basis the problem of induction in its traditional form is a pseudo-problem; and (4) that the principle of induction must be interpreted as a *pragmatic* or *operational* maxim.

I

Let us begin with a brief summary of those views concerning the problem of induction which seem to have received the most general approval among contemporary logicians and philosophers.

(1) Induction is essentially different from deductive inference. It can never attain certainty. All attempts to transform inductive into deductive inference fail because they necessitate the introduction of *inductive* premises. Hume has shown that induc-

[1] This paper was read at a meeting of the western division of the American Philosophical Association at Ann Arbor, March, 1932. In what is presented as the constructive part, I am indebted to the criticisms and suggestions of Professor P. W. Bridgman and Professor C. I. Lewis of Harvard University, as well as to my friends of the Vienna circle, especially Professor R. Carnap and Professor M. Schlick.

tion can be proved certain neither on logical grounds nor on the basis of its own success.

(2) Induction is the indispensable foundation of all factual science, although it is admitted that the more advanced factual sciences do not actually proceed by inductive generalization. Their method consists rather in the construction of hypothetico-deductive systems. The strength of such systems lies in the high degree of internal connectness by which the various parts of a system reinforce one another. But logically, if not genetically, a theory is inductive. This is clear from the fact that any verification establishes the truth only of singular or particular propositions but not of general hypotheses.

(3) The principle of induction expresses the increase of the *probability* of inductions in dependence upon the accumulation of factual evidence. Such evidence consists in the elimination of irrelevant circumstances, as well as in the positive confirmation of a specific connection, uniformity, or regularity.

(4) This principle of induction is not a consequence of the purely logical axioms of the calculus of probabilities. It can be demonstrated only on the basis of *assumptions concerning the general constitution of nature.* Thus, Jevons took nature to be something like an urn to which we can apply Bayes' Theorem. Peirce, similarly, assumed that our observations represent "fair samples" of a thoroughly statistical world. Zilsel, Broad, Keynes and Nicod introduce more refined formulations of the "Principle of the Uniformity of Nature" such as the "Principle of Limited Depth and Variety," or at least the antecedent probability of such assumptions.[2]

(5) The quantitative value of probabilities and their convergence toward certainty can be derived only from the presupposition of rather arbitrary and artificial conditions whose fulfilment is by no means warranted in any case of scientifically significant induction. The idea of determining the numerical value of the probability of scientific theories seems preposterous.

[2] E. Zilsel: *Das Anwendungsproblem*, Leipzig, 1916. C. D. Broad: "The Principles of Problematic Induction," Proc. Aristot. Soc., 1927–8. J. M. Keynes: *A Treatise on Probability*, 1921. Nicod: *Foundations of Geometry and Induction*, 1930.

(6) Since these general hypotheses underlying induction are interpreted as significant assumptions concerning the structure of reality, they must be logically synthetic and, therefore, themselves *inductive*. This is the fundamental difficulty. *What can be meant by the probability of these presuppositions of the probability of all particular inductions?* It is held that even these presuppositions can be rendered increasingly probable by the verification of their consequences. Keynes and Nicod believe that they have proved this argument to be free from circularity. To many thinkers the whole issue appears highly problematic.

II

The unsatisfactory state of the problem of induction seems to me to be due to the preoccupation with the problem of the *validity* of induction. The more fundamental question concerning the *meaning* of the principle of induction is rather neglected and it is this which needs a careful, logical analysis.

The chief difficulty, undoubtedly, lies in the interpretation of the concept of probability. To Hume, the probability of induction was a subjective or psychological matter. It was a degree of belief or an intensity of expectation, based on habit. In contrast with this reduction of probability to something irrational and in opposition to the classical *subjective* interpretation of mathematical probability, two types of *objective* interpretations have been advanced:

(1) *Probability as a Logical Relation*—the theory of Leibniz, Bolzano and W. E. Johnson, most fully expounded in Keynes' *Treatise*, and accepted by C. D. Broad, Nicod and others.

(2) *Probability as the Limit of a Statistical Frequency*—the theory of Venn and Peirce, rejected by Keynes, but recently restated, defended and mathematically systematized by the Berlin mathematician, R. v. Mises.

According to the logical interpretation, which still seems generally favored, probability is the relation of partial or inconclusive implication between one proposition and another. But this account is for Keynes merely a characterization, not a definition, of the fundamentally unanalyzable and indefinable probability

relation. Here, I believe, Keynes is fundamentally in error. I do not mean to deny that the probability relation can be chosen as a primitive notion in an axiomatization of the probability calculus. That can of course be done, and has in fact been done by Keynes, and more recently by Reichenbach.[3] But probability is also applied to empirical facts and in this case we need rules in order to determine the value of the applied probability. These rules, if they can be stated at all, are then the *definition of the empirical or applied probability concept.* I have not time here to prove in detail that in any significant application of the probability concept its essential meaning is statistical. However, I shall discuss a few of the relevant points.

If the principle of indifference operates not on the basis of equal ignorance, as did its predecessor, the "principle of insufficient reason," but on positive grounds, it must inevitably make use of statistical assumptions. Often these assumptions are tacit and in many cases their statistical character is not recognized, but they are the true source of every fruitful probability argument. The "indifference" or "irrelevancy," which is the crucial concept in the principle, means generally *causal irrelevance.* Causal irrelevance, however, is identical with random distribution, and random distribution is a fact which can be established only by statistical investigation. The essential and final test for the correctness of any estimate of probabilities is always the comparison with the statistical frequencies. Confronted with an "a priori" probability, one can take only one reasonable attitude, and that is to ask the direct question: What bearing does it have on observable facts? It is understandable that Peirce, who introduced this pragmatic question as a general criterion of meaning, was at the same time perhaps the most convincing advocate of the frequency theory.

III

If probability is to be a significant guide for our expectations and predictions, inductive probability must be interpreted in terms of the frequency theory. Once it is granted that induction

[3] H. Reichenbach: Axiome d. Wahrsch. Math. Zs. 1932.

is not an infallible procedure, all our care must be directed toward attaining success at least with a maximal frequency. Mill's famous question—Why are the experimental methods (as stated in his own canons) much more efficient than induction by pure enumeration?—means precisely: Why are the experimental methods successful more frequently than simple induction in the discovery of a law? The fact that they are more successful has stimulated the desire for a more profound explanation in terms of assumptions about the structure of the world. The assumption that there are at the bottom of nature strictly deterministic laws, on the one hand, and complete independencies on the other, seemed to account for the superior reliability of experimental methods. For if we follow the simpler method of pure enumeration, we can always be deceived by strong statistical correlations which we may mistake for laws. Only the experimental methods are capable of splitting up such correlations into their strictly causal and strictly random components.

This hypothesis of the "All or None" character of nature has been one of the most fundamental and fruitful guiding principles in almost every field of science. But the recent development of Quantum Physics has proved that it is not an a priori or necessary truth. According to Quantum Physics some of the elementary laws of nature are statistical correlations which will probably never be reduced to a deterministic scheme. The change in attitude is fundamental, and even if determinism should be reestablished as a result of surprising new discoveries, the lesson taught by modern physics would remain of great importance to the theory of induction. Any assumption or "Inductive Hypothesis" (in the sense of Broad and Keynes) about the general constitution of nature is subject to possible correction in the light of new experimental facts, and can therefore be regarded only as a tentative frame-work for more special research. Any such assumption is simply one of an infinity of possibilities, and unless it is accompanied by still more general and precarious suppositions, it can not be assigned a finite probability. On this point the theories of Keynes, Broad and Nicod are seriously in error. Even on the basis of the logical interpretation of probability, the as-

sumption of the finite antecedent probability of an "Inductive Hypothesis" is untenable. These able thinkers are mistaken when they assert that a singular fact can confer a finite probability upon a general assumption. This is possible only by the exclusion of alternative assumptions. Therefore, the whole issue is prejudged. It can never be demonstrated that the principle of induction has the faintest probability except by a petitio principii.

But even if we accept the "Principle of Limited Variety" as a necessary condition of induction, it is easy to see that it is by no means sufficient. Unless we are allowed to infer from the probabilities of the chance coincidences of causally independent characters (or events) something concerning their corresponding frequencies, the principle of induction can have no significance. It is precisely the assumption of the stability of statistical frequencies which is necessary here. But of this assumption we can never be sure. The occurrence of a long chain of extremely improbable coincidences can always mislead our inductions. And there is no way to make sure that we are not living in just such an unfavorable world epoch. If we actually believed that we were so situated we would terminate all investigations and wait until the world passed into a more propitious stage. But the peculiar fact is that we are optimists, and refuse to abandon the belief that we can obtain "fair samples" of the world.

The probability of induction is therefore established on the basis of generalizations for which there is no probability at all. These generalizations extrapolate statistical frequencies, but only more special hypotheses can acquire probability with reference to such frequencies. The probability of a natural law is determined, roughly speaking, by the success-frequency of the inductive method by which it was discovered. The principle of induction, formulated in terms of the frequency theory, states simply that those regularities which have held so far without exception will be found to hold most frequently in the future.[4] According to this analysis, the probability of induction is always secondary and

[4] It should be noted that the frequency theory is still in process of completion and reconciliation with the logical theory. There are difficulties, but they do not appear insurmountable.

hypothetical, and can never be a genuine attribute of pure generalization.

But if, as we have seen, this most general presupposition of all induction cannot be shown to be appreciably probable, is there any other justification for accepting it? The usual reply—and here the influence of Kant is noticeable—is that such assumptions are necessary conditions for the possibility of knowledge in general. It is true, of course, that knowledge of nature would be impossible if there were not a certain amount of order and simplicity. But what are we to infer from this? That in our scientific investigations we must always begin with the postulate or demand of order and simplicity? But obviously it does not follow from the fact that we demand something that we get what we demand!

Our critique must seem very destructive, and it is destructive as regards illusory solutions of the problem. After the failure of all these attempts to achieve anything like an objective vindication of induction are we finally driven back to Hume's scepticism?

There are thinkers, however, who deny that Hume's analysis of causality and induction has any sceptical consequences. R. E. Hobart[5] has most convincingly shown that Hume's arguments appear sceptical only to those who desire to prove what cannot possibly be proved. Moreover, he has shown that almost everything that we call reasonable, rational or justifiable in our active life is absolutely dependent upon belief as the ultimate basis of all our significant knowledge. This ultimate belief, according to Hobart, is present as an immediate fact in every cognitive situation. Although I fully agree with him, yet I feel that for the purposes of a logical analysis of knowledge his formulations are not adequate. For, as he admits, to speak of belief is to speak in terms of psychology. Yet psychology itself is possible only through the belief in induction. Every explanation of belief in the context of psychological or biological theories presupposes induction, because it is by induction that we establish explanations. It is quite legitimate to study the phenomenon of belief from the scientific point of view, but in a systematic logical account of the

[5] "Hume Without Scepticism," *Mind*, 1930.

structure of knowledge the principle of induction is prior to the recognition of its embodiments in psychological or biological processes.

IV

What then is the nature of this principle? Its peculiarly elusive character is startling. If it is a meaningful assumption about the world, then it is no longer the most general principle of induction, it is itself inductive. And if it is stated in such terms that it can never be verified or proven false, then it does not say anything at all. How can we escape this dilemma?

A glance at the logic of deduction will provide us with an instructive analogy. In any axiomatic, deductive system the starting point of our deductions is a set of primitive propositions or postulates whose truth is either "evident" or assumed. From these we derive other propositions. But in order to do this we must have methods or rules of deduction. Important examples are the Rule of Substitution and the Rule of Inference. The one allows us to substitute logically equivalent terms for each other, the other allows us to drop true premises and assert the conclusion. These rules are not commands, but anyone who wants to perform deductions must employ them. (Professor Sheffer of Harvard calls these rules "prescripts," in contradistinction to the postulates which are "descriptive" either of facts or of logical structures.)

Analogously, the principle of induction is not a bit of knowledge, it is neither analytic nor synthetic, neither a priori nor a posteriori, *it is not a proposition at all. It is, rather, the principle of a procedure, a regulative maxim, an operational rule.*

According to the viewpoint of logical analysis, all empirical knowledge is a construction erected upon immediate experience. What this immediately given really is can be disputed, but that there must be some such "groundfloor" of knowledge is necessary if any empirical proposition is to have a meaning. If it is the possibility of verification which establishes meaning, then verification itself must consist in the comparison of elementary or atomic propositions with the given. These elementary propositions are the

raw material of knowledge. Moreover, as in the case of deductive systems, inductive science too has its prescriptive rules, and the principle of induction is undoubtedly the most significant among them. Its nature, just as the nature of the rules of deduction, can be determined only through the recognition of the function that it fulfills with regard to the goal of science.

Now the ultimate goal of science is not the achievement of a loosely connected miscellany of descriptions, but the establishment of a systematic structure of laws as a basis for explanation and prediction. The prescriptive rule, which is a direct consequence of this objective, is then the real principle of induction. It reads: "*Seek to achieve a maximum of order by logical operations upon elementary propositions. Generalize this order (whatever its form be: causal, statistical or other), with a minimum of arbitrariness, that is, according to the principle of simplicity.*" The condition of simplicity is essential, because it restricts the ambiguity of the procedure. But, since simplicity is measureable, if at all, only with great difficulty, there will usually be several ways of generalizing. This explains the case of competing scientific theories. Only when new experimental evidence is supplied, can it be determined that the one or the other theory is more complicated in that it employs more arbitrary hypotheses.

If foreknowledge is to be distinguished from arbitrary or capricious guessing, if it is to be different from dream and inspiration, no other definition can be given of the procedure of science. However, the principle does not carry in itself the guaranty of its own success. In this it is radically different from the rules of deductive inference. Here the analogy breaks down. Hume's scepticism is irrefutable if it simply emphasizes this difference. But with regard to operational rules doubt has no meaning. As long as there is knowledge in the sense in which we have hitherto understood knowledge, the principle of induction will be its inescapable guiding maxim. This is in itself an analytic proposition, the sheerest tautology, because it merely makes explicit the definition of knowledge. The attempt to know, to grasp an order, to adjust ourselves to the world in which we are embedded, is just as genuine as, indeed, is identical with, the attempt to live. Con-

fronted with a totally different universe, we would nonetheless try again and again to generalize from the known to the unknown. Only if extended and strenuous efforts led invariably to complete failure, would we abandon the hope of finding order. And even that would be an induction.

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NOTE

Questions and criticisms of those who have read the manuscript of this paper made it clear to me that it is too condensed to carry conviction. It would have to be expanded into a little book (which I hope to present sometime) in order to fully substantiate both the critical and the constructive tenets here set forth rather dogmatically. My consolation for the meantime then must be: Sapienti sat.

I wish to state also that essentially the same solution of Hume's problem as suggested here has been expounded most recently by Prof. H. Reichenbach in Erkenntnis, vol. 3, pp. 421–425. It is particularly gratifying to me that Prof. Reichenbach, after an Odyssey of attempts to found induction on probability has finally recognized our (Viennese) criticisms and is joining us now in the pragmatic view of inductive generalization.

Finite or Infinite?

BY

E. T. BELL

I

WHEN I undertook to write an article for mathematical laymen on the mathematical infinite. I did not realize the depths of my own layness, I do now. Having refreshed my memory of the classics of infinity by re-reading among other things the famous papers of Cantor and Zermelo, and having struggled like a boa constrictor to swallow the latest papal bull on the human significance of the infinite, I am completely reduced to what Professor E. W. Hobson aptly and somewhat indignantly calls Mathematical Nihilism.

This latest authoritative utterance which annihilated me is the remarkable triad of lectures by Hermann Weyl, *The Open World*, which begins with "A mathematician steps before you, speaks about metaphysics and does not hesitate to use the name of God," and which ends with something that possibly God alone understands. So far as I can make out, the paradoxes of the mathematical infinite furnish an irrefutable proof of the existence of Jehovah. Whether it was indeed Professor Weyl's aim to add another proof to the large number—practically infinite—which have been given in the past 1900 years, I do not know; the reading of the lectures left me with the impression that such was his intention.

The foregoing remarks are not irrelevant. It so happens that before Professor Weyl's book appeared, I had written what any

impartial critic might think was intended as a parody of Weyl's position. Or, if it had been the other way about, his position might have been construed as a parody—and a much better one—of mine. We had referred to the same prophets of the infinite, including in particular Pythagoras, Schemhamphoras, and Nicolas Cusa. To see Nicolas rising from the dead in 1932 gave me a shock from which I have not yet recovered.

Having tried to understand Weyl's position on the philosophy of the infinite, and having failed, I began to doubt whether I have ever understood anything about the mathematics of the infinite, in which Weyl is one of the leading authorities. It is not necessary to elaborate any of these doubts; a few quotations from the classics may suggest sharper suspicions to any who are not congenital believers. It is at least a tenable position that the modern theory of the infinite is a theological hangover from the Middle Ages. This, of course, does not necessarily make nonsense of mediaeval theology, whatever it may do to the claims of those sturdy mathematical skeptics, mostly analysts, who profess an open contempt for all theology.

We shall not get very far into this vast subject. Certain plebeian words, like ratio, class, irrational, choice, number, correspondence, and others that infest the theory of the infinite are less clear than they were thirty years ago, and before we can begin to understand where the present day problems of the infinite lie, we must recall at least a few of the things that have been said about the meanings of these words—if any of them have any meaning other than what we intuit. Instead of writing on the infinite as such, I shall confine my efforts in this article to an attempt to describe those peculiarities of numbers which brought the infinite into mathematics. Then perhaps on some future occasion I may venture into the deeper waters of the infinite itself. If anyone objects that all the doubtful points have been satisfactorily disposed of by more recent writers, he may be reminded that there is a fatal lack of agreement in the solutions proposed for the difficulties. This may be less serious than appears at first sight: The ultimate way out may be through one of the recent attempts to construct logics based on many-valued "truth."

II

Tradition ascribes the creation of mathematics as an autonomous enterprise to Pythagoras and his school (Sixth Century B. C.) Recent historical research tends to throw the beginning farther back, and to suggest that Pythagoras was anticipated in his stringent requirement that even the most obvious mathematical theorem be deduced from postulates or axioms.

The other outstanding achievement of the Pythagoreans was the invention—or discovery—according to one's mathematical creed—of irrational numbers. This awkward invention started the theory of the mathematical infinite—analysis, controversies, paradoxes, polemics, and all. That two strides of such magnitude were taken in a single century, possibly in the lifetime of one man, is without parallel in the history of mathematics. Without the postulational method mathematics simply does not exist, and without a consistent theory of irrational numbers mathematical analysis hangs by nothing in the middle of nowhere.

Before the meaning of irrational number can be grasped, we must understand what rational numbers are. This in turn demands a glance at integers and ratio. Let us take the last first.

Ratio, to quote Euclid, is the mutual habitude of two magnitudes of the same kind with respect to their quantuplicity. Every word in this definition will be found in the dictionary, and the sentence is grammatical. Let us pass over ratio and assume for a moment that we know what is meant by the integers 1, 2, 3, 4. . . . We shall return to ratios, but not to ratio, presently.

Knowing what the integers are (we shall describe them also after a modern fashion later) we can state what a rational number is. A rational number is the ratio of two integers. We can ignore negatives for the moment. In common language, a rational number is the result of dividing one whole number by another. The integers are the rationals with divisor 1.

Any mathematician writing on mathematics expects a certain amount of decent skepticism on the part of his reader, unless possibly the reader is another mathematician as credulous as himself. The definition which I have given of rational numbers should satisfy nobody—layman or mathematician. Let us

replace it by one which many mathematicians accept, still assuming that we know what is meant by the integers 1, 2, 3, 4. . . . The need for such a revision is apparent from the two examples $\frac{3}{4}$, $\frac{4}{3}$, the first of which is .75 and the second, in the same scale, is 1.3333 . . . , where the decimal part .3333 . . . is *non-terminating*. One positive gain of the following restatement is that it avoids all non-terminating or infinite processes. The spirit of it goes back to Gauss (1777–1854).

Instead of writing $\frac{4}{3}$ for the ratio of 4 to 3, and then trying to understand what $\frac{4}{3}$ means by converting it into an infinite decimal, we merely write (4, 3) for this *rational number*, putting the *numerator first*, and the *denominator second*, in this *ordered couple*, or *ordered number pair* of *integers*, 4, 3. So far we have merely told how to make a compound mark out of given marks; here the marks are 4 and 3 and the comma and the parentheses. We now define the *ratio of m to n* (notice, not of *n* to *m*), where *m*, *n* are any integers greater than zero, or the rational number m/n, to be the *ordered pair* (m, n), *provided such pairs* (m, n) *are by hypothesis made to satisfy certain postulates which state the rules for combining such pairs.* These rules may seem artificial at first, but they are simply the common school arithmetic of fractions restated in a fashion which, optimistically enough, is intended to be precise and free from logical objection.

Postulate (I). *If a, b are any given positive integers, the ordered pair,* (a, b) *is uniquely defined, or uniquely known.*

Definition. $(a, b) = (c, d)$ means $a = pc$, $b = pd$, where a, b, c, d, p are positive integers; and (pc, pd), (c, d) are the same.

Postulate (II). *If* (a, b), (c, d) *are any pairs as defined in* (I), *then*

$$(a, b) + (c, d) = (ad + bc, bd),$$

and

Postulate (III). $(a, b) \times (c, d) = (ac, bd)$.

Definitions. $(ad + bc, bd)$ in (II) is called the *sum* of (a, d), (c, d), and (ac, bd) in (III) their *product.*

There are several other postulates, particularly those which define the relations of greater and less for such pairs, but for our purpose the specimens above are sufficient.

Any thoroughgoing criticism of (I), (II), (III) would no doubt land us in some of the more serious objections which modern mathematical Nihilists have raised against the theory of the infinite itself, and here we are still in the strictly finite domain. There is so much to be said that no one could hope to say it all in any reasonable compass, and we must confine our attention to a few of the objections which go over, identically in spirit if not in words, to the modern criticisms of the doctrine of the mathematical infinite.

There are, I believe, serious difficulties in the foregoing postulation of our familiar fractions, and until these are satisfactorily cleared up, I see no point in proceeding to infinity in any roughly similar direction. The postulates, by the way, are transcribed (except the first, which was overlooked or taken for granted) from an authoritative and critical work on mathematical analysis.

To clarify the postulates for those whose trade is not the juggling of mathematical symbols, I rewrite (II), (III) in their more familiar forms, which give the rules for addition and multiplication of fractions,

$$\frac{a}{b} + \frac{c}{d} = \frac{ad + bc}{bd}, \qquad \frac{a}{b} \times \frac{c}{d} = \frac{ac}{bd},$$

in which *ad*, *bd*, *ac*, *bd* mean *a times d*, *b times c*, etc.

Why not say simply that $\frac{4}{3}$ is the result of dividing the 'unit' into 3 equal parts and then taking 4 of these parts, as we did at school, instead of mystifying it as (4, 3)? The answer is a long story that must be passed over. It is sufficient to state that this easy way is so full of pitfalls that not even the most conservative reactionary would now dare to take it, to say nothing of Nihilists. We write (4, 3) and postulate (I), (II), (III), because we are trying to understand with precision exactly what it is that we think we are talking about. This entirely elementary matter is worth anyone's closest attention. If it is once thoroughly understood, more than one fog hanging over the mathematical infinite stands a fair chance of being dissipated.

Only a careless mathematician would pass (II), (III) in the form in which they are stated—with care, as I remarked—by the

author of the treatise from which they are taken. The signs or marks in a mathematical statement are supposed to be used unambiguously. Notice that the first definition and (II), (III) do not meet this common-sense requirement. In (II), for instance, the sign + has two different meanings. This is rather better than a similar performance by a professional analyst in another standard treatise on the real variable, where the sign of equality occurs with eleven distinct meanings on one page!

The lapse just noted is a minor blemish—here, but not in similar situations in the infinite—which a little care may remove. The oversight with regard to (II), (III) was one which any mathematician might easily make, perhaps deliberately, especially if he were apprehensive of being called a meticulous quibbler or a mathematical Nihilist. Here it serves the useful purpose of rousing our suspicions about (I), which was omitted.

Postulate (I) states that the number pair (a, b) is *uniquely defined or uniquely known*. The italicized phrase conceals a central difficulty. Before trying to realize what that difficulty is, let us recall that many professional mathematicians object vehemently to any attempt to go behind mathematical symbols and discuss their meaning, or their knowability, or even their thinkability. When we come to classes it will be evident (definition (A) in IV below) however that the founder (Cantor) of the theory of the infinite had no such scruples. For better or worse we shall assume that Postulate (I) is necessary to a discussion of our ordered pairs.

To put the difficulty in as exasperating a form as possible, *how are we to know whether it is* (a, b) *or* (b, a) *which is before us for contemplation?* A quibble, says the formalist. Granted. What the quibblers wish to see is the quibble un-quibbled, or a proof that will withstand other quibbles that by ignoring such quibbles as that concerning (a, b), (b, a) the formalist does not quibble himself into a quagmire from which he cannot escape with his boots or even his breeches.

Of recent years a new school of mathematical thought has arisen, most vigorously in Poland, to supplement the German formalists led by Hilbert, the English logisticians who follow

Russell and Whitehead, and the intuitionists of the Dutch school headed by Brouwer, in their various attempts to talk sense about mathematics. Wittgenstein, of whom Russell thinks highly, fits in somewhere in this list; the new school apparently does not, although it is a natural sequel to the formal and logistic approaches. It may be called the semantic school, where 'semantic' has its common dictionary meaning.

One tenet of the semanticists appears to be that a *sign*, like (a, b), can be distinguished from another sign, like (b, a), say. Granting this, we no longer have any difficulty with Postulate (I). But, in granting it, we have lifted the lid of Pandora's box, and have let out all the annoying things the non-quibblers have tried to keep locked up.

It is surely unnecessary to labor the point that Postulate (I) in implicitly affirming that (a, b), (b, a) are distinguishable pairs when a, b are distinct integers, has appealed to human physiology and psychology. Any expert in geometrical optics could easily design a pair of eyes with a sort of pulsating cylindrical squint which would always see (a, b) and (b, a) as the same pair. Eyesight having failed as a discriminating test, we might appeal to sound and language. But then a competent mechanic or a judicious use of alcohol and hashish might well tie our tongues into trefoil knots and our ears into French horns to which ab and ba would be one and the same. Smell, I believe, has not yet been suggested as an aid to the human comprehension of vulgar fractions; it is said to work miracles with intelligent dogs.

The upshot of this seems to be that pure formalism in mathematics raises at least two difficulties for every one which it dispels. A little more technically, it is not at present feasible to construct mathematics, or even the metamathematics of Hilbert, in which the same distinguishability of signs is tacitly assumed, without appeal to what physics calls the observer. The human element is there whether we agree to recognize it or not, and the day of absolutes is past in mathematics as in physics. This I must emphasize is not the position of many who are entitled by their creative work to a competent opinion; for many *discovery*, rather than *invention*, is the correct word in describing mathematical advances.

There is a definite and unbridgeable gap here: some *discover* the integers, the rationals and the rest, precisely as Plato would have had them do, as more or less shopworn images of second or third hand ideal numbers; others *make* the numbers out of their psychological human behaviour, and are not concerned with eternal essences. The like goes over verbatim to the classes, aggregates, assemblages or whatever we choose to call them, of the mathematical theory of the infinite. In what sense are the integers or the points on a line given? If they are not given, and if we are thereby forbidden to use the convenient mathematical appeal 'there exists' in our proofs, how are we to *construct* integers, classes, points and the rest in a manner which will fit our habits of reason, or possibly suggest better habits?

The attempt to explain what common fractions are as ordered pairs of positive integers has been a dismal failure. Perhaps a more technical account might make everything clear? In my opinion it would not; the deeper we venture into the underbrush of technicalities the dimmer becomes the trail, and at every step the possibilities for going astray double. If we cannot start right we are not likely to get anywhere in particular. This may seem a queer conclusion for a working mathematician to reach. But my capacity for belief on insufficient evidence has dwindled after years of trying to follow kaleidoscopic fashions in mathematical philosophy. Nevertheless, like the majority of working mathematicians, I try to keep working, philosophy or no philosophy.

Nearly all agree that we must start somewhere, and many prefer to start with the positive integers as 'given'—by 'the good God,' according to the ungodly Kronecker. But this common agreement is no guarantee that the parties to it will not presently find themselves in a lawsuit. And so, ultimately, mathematical philosophy—the brilliant work of Russell on cardinal numbers, for instance—is justified of her children who turn their backs on her. Sooner or later, it is a reasonable guess, they will be found out, even if they have to do the finding themselves.

Having failed to understand the meaning of vulgar fractions as number pairs, I take them for granted and go on in a moment

to something that looks easier because it is closer to the infinite. Negative numbers were ignored in the preceding account. One modern way of basing negatives directly on the positives, due to Kronecker, again uses the device of ordered couples, and hence is open to any objection that can be raised against the sophisticated revision of vulgar fractions. All, as was previously remarked, go back in spirit to Gauss, who introduced the device of ordered couples to put complex numbers on what, in his day, was a firm epistemological foundation. For the rough and ready work of technical mathematics it is still no doubt firm enough; for any but complete formalists in mathematical philosophy it requires re-examination.

Before passing to the next I should like to record a recent incident at a meeting of professional mathematicians, to indicate that preoccupation with the subtleties of 'ordered pairs' is not exclusively the affliction of those who peer at the foundations of mathematics, but is also a matter of concern to working mathematicians. In his endeavor to understand what is meant by an 'indeterminate,' as used in the theory of numbers, and in his efforts to learn in what sense every algebraic equation has a root, one of the lecturers had been driven—not by his own volition, but by his erudite friends—to examine the work of Gauss on number pairs. His comment on the outcome of his examination was illuminating—from a professional mathematician: "I was disgusted." The lecturer in question has made outstanding contributions to the severely technical side of one of the thorniest, strictly mathematical questions in the whole theory of numbers. That subject, by the way, offers excellent training for those who intend later to specialize in the infinite. For there a single oversight spells disaster. The tempting generalizations which present themselves on a close scrutiny of numerous special instances are frequently found to be untrue.

The incident of the professional's publicly expressed disgust is indicative of the breezy irreverence which characterizes the younger generation. The mere ipse dixit of any master is an insufficient retort to current doubts. The fathers have eaten green persimmons, and the children's teeth are on edge.

"Show us the Father, and it sufficeth us" is a not unreasonable demand, in mathematics as in the place where it was first uttered. Kronecker repeated it in mathematics. In this spirit let us approach the infinite through the rational integers, after a swift glance at the classical theory of proportion.

III

"The crown of Greek thought" is the theory of proportion in Book V of Euclid's Elements. The cardinal definition (the fifth in Book V) is the cornerstone of it all; it was devised by Eudoxus (408–355 B.C.).

The discovery of the Pythagoreans that not all geometrical magnitudes are expressible as rational numbers upset most of what the Brotherhood thought it had established concerning similar figures. To take a simple example: Let a straight line drawn parallel to the base BC of a triangle ABC cut the sides AB, AC in the points P, Q respectively. *Then the lengths of the segments AP, PB, AQ, QC are the four terms of a proportion*, namely

$$AP : PB :: AQ : QC;$$

or, in another script,

$$\frac{AP}{PB} = \frac{AQ}{QC}.$$

This is the fundamental theorem in the geometry of similar figures.

Anyone who learned his elementary geometry in almost any American school during the past thirty years should appreciate the next, for he most likely will have got no further in proving the above proportion than Pythagoras did. The interesting part of the proof, the 'incommensurable case,' was probably relegated to the Appendix with the other important 'Supplementary Topics,' in accordance with one modern pedagogical theory that all young persons are imbeciles incapable of half an hour's close thinking. Without the 'incommensurable case' the theorem is gruel.

The Pythagoreans had supposed that $\frac{AP}{PB}$ and $\frac{AQ}{QC}$ when ex-

pressed numerically in the theorem are always *rational* numbers—just as the average product of any emasculated high school course does today. They did not know that *irrational* numbers exist. Stated otherwise, they assumed that it is possible always to find a *common measure* for the lengths of any two straight line segments such that each of the segments is *a whole number of times this common measure.* Pythagoras or one of his collaborators discovered the falsity of this assumption. In particular, it was proved that a side and a diagonal of any square have no common measure, or are incommensurable. Thus, if a side is 1 unit long, a diagonal is $\sqrt{2}$ units long (by the Pythagorean proposition concerning the square on the hypothenuse of a right triangle), and the fact is that it is impossible to get the square root of 2 by dividing one whole number by another.[1]

So much for the discovery (or invention) of irrational numbers, which begot the mathematical infinite—except possibly Plato's comment on the discovery, which might be of interest to some of our educational doctors. Plato declared that those who do not know of the existence of irrationals are not men but swine. Today we would sidestep the vivid word, even when addressing mathematical Nihilists at a Teachers' Institute.

To take care of the incommensurable case, and so incidentally give a universally valid proof of the triangle proportion, it was necessary to rebuild the Pythagorean theory of proportion from the ground up. The foundation of this rebuilding is the *definition*, by Eudoxus, of *proportion.* To bring out the kinship of this

[1] An old proof of this may be outlined thus. If $\sqrt{2}$ is the ratio of two whole numbers, a, b, then $\sqrt{2} = a/b$. If a, b have a common factor greater than 1, it can be cancelled from both a and b without altering the value of a/b, so we shall assume that a, b have no such factor. Square both sides of $\sqrt{2} = a/b$. Then $2 = a^2/b^2$, and hence, multiplying throughout by b^2, we have $2b^2 = a^2$, where a, b are whole numbers with the *greatest* common divisor 1. Since $2b^2$ is even, and $2b^2 = a^2$, a^2 must be even. Hence a is even; say $a = 2c$, where c is a whole number. From $2b^2 = a^2$ we now get $2b^2 = (2c)^2$, or $2b^2 = 4c^2$, or $b^2 = 2c^2$. Repeating the argument we get b even; say $b = 2d$. So, in the original $2b^2 = a^2$, we have $b = 2d$, $a = 2c$. That is, b, a have the common divisor 2, contrary to the assumption that 1 is their *greatest* common divisor.

In this sketch many subtle properties of arithmetical divisibility are presupposed. These were all proved by the Greeks and expounded by Euclid. Unless the reader is experienced in this kind of thing, some of the presuppositions will perhaps have passed unnoticed. But they are all justifiable by strict reasoning; nothing has been put over.

definition with some of the modern doubts on the infinite, I have capitalized certain parts of it.

"Magnitudes are said to be in the same ratio, the first to the second and the third to the fourth, when, if ANY equimultiples WHATEVER BE TAKEN of the first and third, and ANY equimultiples WHATEVER of the second and fourth THE former equimultiples alike exceed, are alike equal to, or are alike less than, THE latter equimultiples taken in corresponding order."

Sir Thomas L. Heath, from whose Manual of Greek Mathematics (Oxford, 1931) this translation of the definition is taken, remarks that "Perhaps the greatest tribute to this wonderful definition is its adoption by Weierstrass as a definition of equal numbers."

More might have been said with equal justice: Weierstrass, who with Dedekind and Cantor created the modern theories of irrationals and the mathematical infinite, followed Eudoxus in accepting the capitalized words and all that they imply as entirely unobjectional, whereas such an old timer as Isaac Barrow (1630–1677, Newton's professor at Cambridge) objected most vigorously to them, and thereby anticipated the latest Nihilistic objections to Cantor's definition of class (Menge), the like for the mathematical definition of continuity, and the older but recently rejuvenated objections of Kronecker to mathematical existence proofs that do not prove existence by constructing or exhibiting the 'existee.' In this connection Kronecker declared that 'the square root of 2 exists' is a meaningless statement; he denied that there are such things as irrationals, and he sketched a means for circumventing their use, which is now becoming a commonplace—so far as it has gone—in fairly elementary mathematical instruction, especially in modern algebra. Passing this however, it is enough to state here that attempts to put mathematical analysis (the differential and integral calculus, the theory of functions of a real variable, etc.) on a firm foundation led Weierstrass and others to a critical re-examination of irrationals, and hence back to the beginning made by Eudoxus in the Fourth Century B.C.

Barrow's Nihilistic attack on Eudoxus' definition has a strangely modern ring. He objects to the definition in effect

because it seems to appeal to an indefinite or infinite number of acts of choice exercised upon a totality of which we know nothing and can have no experience; he further objects to the lack of a definite criterion by which the equality or inequality of the equimultiples furnished by this grab-in-the-dark method of choosing them could be decided. Without excessive quibbling mathematical Nihilists refer Eudoxus' definition to the Nephelococcygia of Platonic ideas and patristic absolutes. If such a reference be just, the definition ceases to be a part of mundane mathematics and soars to the loftier realm of theology. But, unless I have misunderstood Weyl's position completely, this apotheosis is no more than could have been expected. Mathematical readers may be interested in applying Barrow's strictures to the modern, rigorous definition of a limit as given in any standard text on analysis.

IV

An attempt to understand what a cardinal number is brings us closer to the infinite. Russell's definition seems in fact to conceal some of the difficulties inherent in Cantor's conception of an aggregate which, according to doubters who are first of all professional mathematicians and only incidentally mathematical philosophers, is no longer tenable.

The older definitions by the Greeks, 'a number is a collection of units' (Thales), 'a number is a determinate multitude' (Eudoxus), still survive in simpler terminology in elementary instruction. They are unobjectionable and equally meaningless; so formalists may relish them. If there is anything unsatisfactory about them it is principally their trick of deluding unsuspecting children into thinking they understand something where nothing is understandable. Possibly the sophisticated definition (C) given presently may be found by our immediate successors to suffer from similar disabilities. All good mathematical Nihilists will rejoice in the preliminaries (A), (B).

(A) "By a 'Menge' we understand a collection of definite readily distinguishable objects which through our intuition or our imagination we view as a whole." (Cantor.)

When Pythagoras (or the devil) invented philosophy for the subsequent confusion of mathematicians he must have foreseen the German language. Subtle shades of meaning vanish in translation. A literal translation is intolerable in English, and a readable one is hopelessly inexact. For these reasons I give (A) again in Cantor's own words.

(A) "Unter einer 'Menge' verstehen wir jede Zusammenfassung von bestimmten wohl unterschiedenen Objecten unserer Anschauung oder unseres Denkens zu einem Ganzen."

In briefer and possibly sufficient English the meat of it is

(A) *A collection of definite well defined objects which is contemplated as one whole* is called an aggregate.

English equivalents of 'Menge' are *aggregate*, *set*, *assemblage*, *class*. We shall use the last, although the English school of Russell seems not to be interested in a formally stated equivalent for (A).

A recent tendency is to admit that (A) makes a gratuitous appeal to slippery human elements, and simply to accept *class* as an indefinable whose significance we grasp intuitively. Whether we do this or adhere to (A), it is clear that we have the same psychological problems as we met in ordered couples, but in a subtler and more aggravated form. Consider for example the detailed, word-for-word application of (A) to the set of all points constituting a finite segment of a straight line. I believe it is correct to say that current criticisms of the mathematical infinite are concentrated most strongly on just this definition (A). Unless we understand it or agree to take it for granted and assent to any inconsistencies which it may conceal, we shall get nowhere. So we may as well swallow the camel and proceed to the gnats.

(B) Two classes are said to be *similar* when their *elements* (the 'objects' according to (A) in each) can be placed in *correspondence* so that to each element of either class there *corresponds* one element, and only one, of the other class.

For example, the two classes, x, y, z and a, b, c are similar; the correspondence can be effected by pairing x with a, y with b, z with c, among other ways.

I shall not attempt to state what is meant by 'correspondence,' as

all efforts with which I am acquainted to make the intuitive notion any clearer only pile confusion on mystery.

For its application to infinite classes, (B) has been criticized on account of the words *can be placed*, or their equivalent. How is it to be done, and if it can not be done, or if we can neither prove nor disprove now that it can never be done by any constructive process, are we reasoning consistently when we postulate the possibility of a correspondence and deduce theorems from our postulate? What is the status of the theorems? Are they true, are they false, or are they nonsense? To proceed, we can now define a cardinal number.

(C) "The number of a class is the class of all those classes that are similar to it." (Frege and Russell.)

This definition has been cited because it definitely refers the concept of a number to that of a class, and classes also are fundamental in Dedekind's definition of irrationals. Thus all the numbers are thrown back ultimately on (A), or on our intuitive conception of a class, and at least an apparent unity is attained.

Poincaré criticized (C) on the ground that it is circular: to define the number 3 by (C) presupposes complete knowledge of all classes containing 3 things each, and these cannot be recognized unless we know the "3-ness" of a class when confronted with it. This objection can not be disposed of in any brief non-technical discussion. But it has been satisfactorily met, I believe. Anyhow, I know of no substitute for (C) which is free from obvious objections.

It has been urged by some that as we *must* have the integers 1, 2, 3, 4, . . . and the irrationals which mathematicians have manufactured from them in order to make any headway at all in mathematical analysis, we should accept the numbers as brute facts and dispense with all attempts, such as (C) and those of Dedekind for the irrationals, to tame them by 'Sweet Philosophy.' This would reduce the most refined mathematical analysis to the crassest pragmatism, which is just what some of the Nihilists appear to be trying to do, and which is precisely not what the others desire. Advocates of the brute fact program are uncomfortable—when they bother about the facts at all—mainly

because brute facts, true to their nature, insist upon being brutal when common decency would suggest that they be neither seen nor heard. Whoever creates a Frankenstein may expect to be uncomfortable sooner or later.

It is still a long way to the infinite, and the road gets ruggeder and ruggeder. As a first step onward and upward let us recall Galileo's proof that there are precisely as many even integers 2, 4, 6, 8, 10, 12, . . . as there are integers 1, 2, 3, 4, 5, 6, . . . altogether. It suffices to double each integer in the second infinite class—that of *all* the integers—and set up the correspondence in accordance with (B):

$$\begin{array}{cccccc} 2, & 4, & 6, & 8, & \ldots. \\ | & | & | & | & \\ 2\times1, & 2\times2, & 2\times3, & 2\times4, & \ldots. \end{array}$$

The only obvious quibble about this classic is the precise application of (A) to *infinite* classes. Here (A) must be applied to each of the infinite classes 2, 4, 6, . . . 1, 2, 3, . . .; the proof breaks down if either class is finite. Of course if any Nihilist were obstinate enough to object that his private Anschauung and Denken stood deaf, dumb, blind and paralyzed before all numbers greater than a billion to the billionth power, (A) would force him to the conclusion that there are no greater numbers and hence that for him Galileo's proof is invalid. He might then reject (A) and accept the proof on faith, or he might consent to be eliminated from the discussion, or he might agree to let Galileo call him a fool.

As a matter of fact this very objection has been raised against Russell's (C), seriously, I believe. It has been argued that if the universe is finite, as some of the physicists say it is, then (C) gives us only a finite number of integers 1, 2, 3, . . . The World, in the sense of Weyl, is then closed, and all necessity for theological apologetics vanishes. Unfortunately the physical universe seldom stays closed nowadays for longer than six months at a time.

Galileo's proof and its effect on the hypothetical Nihilist seem to indicate that the psychological human elements must be excluded from (A) and from mathematics. But we found indications in a former difficulty that they must be retained. Therefore—. Let us get on with the mathematics and state (D), which is one definition of an infinite class.

(D) An infinite class is a class having a part (*some*, but *not all*) of itself similar (in the sense of (B)) to the whole of itself.

Granted that we understand (A), (B), (C), (D), we shall probably agree that the next is reasonable.

(E) POSTULATE. Infinite classes exist.

There is no need to dwell on existence; a Nihilist like Kronecker or St. Philip might ask to be shown. The postulate (E) circumvents any possible difficulty about the finiteness of the class of all the integers 1, 2, 3, . . . and enables us to state theorems of genuine interest about the elements of this class. For example, the number of primes is infinite. That is, the class of all primes, 2, 3, 5, 7, 11, 13, 17, 19, 23, . . . is similar (B) to a part of itself, although it is extremely unlikely that any human being will ever be able to construct the 2^nth prime, where n is the total number of electrons, protons, and neutrons in any finite universe. Nevertheless this prime is as definite an object unserer Anschauung oder unseres Denkens as the prime 2 is. The last is not a rhetorical exaggeration; it is an exact statement, as we see if we reflect what 2 is according to (C).

Hilbert would say that 'there are an infinity of primes' is an *ideal* theorem, on a different level from '2 is a prime.' Again we seem to have sufficient cause for rejecting the human element from strict mathematics. The formalists go the whole way and reduce mathematics to a meaningless manipulative game with meaningless marks. If tenable, this extreme would at least defer an understanding of ideal theorems to the cloudier land beyond mathematics.

All that I am trying to suggest with (A)-(E) is that some orthodox mathematicians no less than incorrigible mathematical Nihilists find difficulties long before they reach the infinite through the numbers. The next, (G), is a very famous member of the same family. Superficially it seems to have but little to do with mathematics. Nevertheless it has played an important part in the theory of the infinite and in analysis.

(F) A class is said to be *denumerable* if it is similar to the class of all the integers 1, 2, 3, ...

This is merely a verbal definition: if the objects in an infinite class can be counted off 1, 2, 3, . . . the class is denumerable.

(G) It is given that the class M can be split into a denumerable set (or class) of classes, say $M_1, M_2, M_3 \ldots$, which are such that there is at least one object of M in each of $M_1, M_2, M_3, \ldots$ Then it is asserted that M contains at least one part which has in common with each of the classes $M_1, M_2, M_3, \ldots$ precisely one object.

Nothing could be more innocent than this, yet it has led to very serious difficulties in pure mathematics. Effectively it has been used in some proofs in the following form.

(H) A class can be found by choosing exactly one object from each of the classes $M_1, M_2, M_3, \ldots$ described in (G).

This, which is part of the 'Axiom of Choice,' commits us to performing an infinite number of acts of selection and demands, by (A), since the result of our choosing is to be a class, that we shall know what we are doing and be able to grasp the result of our labors in our imaginations. All this without prescribing how to go about any of it; we, as mathematicians, are interested only in knowing that *such a choice can be made*, and we proceed on this assumption. Luckily for our mathematics it has not yet been necessary in any application of the Axiom actually to do the choosing.

It should now be fairly clear that Cantor probably meant exactly what he said in (A). The definition of class was framed with farsighted caution wholly in terms of our mental or psychological faculties and not with reference to our fingers for obvious reasons. However, if the behaviorists should prove to be on the right track, our supposed mental faculties are a myth whereas our fingers are not.

At this point it may prove interesting to glance back and compare (H) with Eudoxus' definition of a proportion.

We have reached the threshold of the mathematical infinite where the technicalities and the real difficulties begin. Before turning back, let us see what an irrational number is. For this last look only the definitions of rational numbers and class are necessary for complete comprehension. The rough definition of a rational number as the quotient of two whole numbers is sufficient to bear in mind; ordered pairs and the rest only obscure the main issue here.

Put into one class, L, *all* the rational numbers (including zero, positives, and negatives) whose squares are less than 2, and into another class, U, *all* the rational numbers whose squares are greater than 2. Then L contains no greatest number, and U no least. Thinking in terms of school arithmetic for a moment we see that the *square root* of 2 is in neither L nor U. Where is it? Kronecker would say nowhere. Ignoring this Nihilistic answer, let us hear Dedekind's.

The classes L, U are said to form a *section*, (L, U), of the rational numbers. It is postulated now that (L, U) *defines, or corresponds to*, some *real number*. Here (L, U) corresponds to the square root of 2; Russell says more explicitly that the section *is* the number.

A similar device is applied to any real irrational number. It works also for the rationals. Hence *all* the real numbers can be viewed as Dedekind sections.

Mathematical analysis and the arithmetic of the infinite begin here. As the Dedekind section has not yet been placed on a satisfactory logical foundation, we shall turn back.

V

After all this, it may be asked why does anyone go on? Every mathematician does, even the strictest finitist or the most violent mathematical anarchist.

As for the Nihilists, I believe that anyone who wishes to be even moderately critical can do as much damage to Kronecker's personal finitist program as he and his school have done to that of his arch-enemy Weierstrass. The present critics of mathematics find many things that Kronecker overlooked, and they raise the devil with his program, precisely as his school and its offshoots have with classical analysis. The analysts are at least aware of the need for careful reconsideration. Even such a mystic notion as that of an indeterminate, which is as hazy as anything in the theory of classes, does not seem to have caused the finitists much discomfort. Forthright Isaac Barrow would not have tolerated the thing anywhere near him.

It has been proposed by extremists that mathematicians give up the infinite. One professional, Borel, says they could get

along without it—probably—but at the cost of interminable prolixity. A less panicky proposal is that mathematicians find out just why it is that they get such interesting theorems, which they believe to be true, by using the mathematics of the infinite.

To most normal human beings it is incredible that a *totally* erroneous theory could attain anything even distantly approximating to consistency. Most mathematicians keep working because experience gives them faith, not in any brand of theology, but in themselves and their ability to find things which have human value, also because they like to work.

The grave difficulties which have arisen in the past forty or fifty years in mathematics may yet prove to be the greatest blessing which the subject has received since Pythagoras died. Although the blessing is well disguised now, we may ultimately see into it and through it, and dispose of it in a convincing manner by really settling the paradoxes which the Greeks and their modern successors have invented for our entertainment and confusion.

On the Foundations of the Theory of Probabilities

BY

D. J. STRUIK

INTRODUCTION

THE foundation of the mathematical theory of probabilities is still a controversial subject. There are schools of insufficient reasoning and of cogent reasoning, of a priori determination and of frequency determination, of subjective and of objective probability. Two main difficulties exist. The first is the definition of equally like events. The second difficulty is the relation between the laws of causal natural science (mechanics, electrodynamics, etc.) and the laws of statistical regularity. Is it really necessary to add to the laws of mechanics one or more independent "laws of large numbers" to explain the regularities in dice throwing? In all these cases different answers exist, and the existence of a mathematical theory of probabilities remains a kind of miracle, as esoteric to the further domain of natural science as the resurrection of the dead.

We shall here attempt a rough and elementary sketch of a foundation of the theory of probability which, at any rate in principle, seems to show the way out. The main ideas are not new, but in their combination seem to be worth presenting. They can be summarized as follows:

I. *Mathematical principle.* The mathematical theory of probability is a theory of measure.

II. *Physical principle.* Statistical regularity is a result of causal relationship. No probability without causality.

We begin with the exposition of the mathematical principle.

1. *The Formal Theory.* It is possible to build up an exact "theory of probabilities" in a purely formal way. In its simplest form, it depends on the following definition of probability:

If a finite number of events, N, is possible and if we are interested in n ($n \leqq N$) of these events, then the probability (or chance) p of these favorable events is

$$p = \frac{n}{N}.$$

For instance, in throwing up a die six cases are possible, namely, that either 1, 2, 3, 4, 5, 6 come up; then, the probability of throwing each of them is 1/6. ($n = 1, N = 6$.)

These N events must be well defined to give a definite answer. If the question is: what is the chance of throwing seven with two dice, we find $n = 6, N = 36, p = 1/6$, because $7 = 1 + 6, 2 + 5, 3 + 4, 4 + 3, 5 + 2, 6 + 1$, six different ways, and 36 is the total number of two combinations of six elements with repetitions, as 11, 12, 13, 14, 15, 16; 21, 22, 23, etc., up to 66.

In the problem of d'Alembert some difficulty arises. What is the probability of throwing a tail in two consecutive throws of a coin? If we denote tail by T and head by H, we can take as possible events TT, TH, HT, HH, of which the first means tail in the first throw, tail in the second throw. Here $N = 4$ and $n = 3$, because three of the four events show at least one T. Hence, in this case, the probability is 3/4.

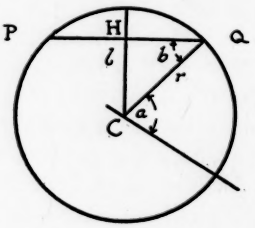
We can, however, also take as possible events T, HH, HT, because when tail comes up first it is not necessary to throw again. In this case $N = 3$, $n = 2$, and the required probability is 2/3,

Both solutions are correct in this formal theory because in both cases the possible events are exactly given. There is no special reason why one solution should be preferred to the other. We may say that in the first case we have defined four "equally likely" cases, and in the second case three "equally likely" cases. This is here merely a complication of terminology. The only thing we need is an accurate definition of the N events from which we are

allowed to choose. Questions of dependence or independence of events do not essentially change this statement.

For N finite (and positive integer) this theory of probabilities is a chapter of the theory of permutations and combinations. For N infinite, either denumerable or continuous, we need other mathematical theories. As an example, let us consider a case of geometrical probability such as the problem of Bertrand.

In a circle a chord is drawn. What is the probability that the chord will be smaller than the radius of the circle? Here, again, a definite answer is possible only when we define accurately what events may happen, and also, what events we are interested in. We introduce the angle a which the radius CQ makes with a fixed direction, and the distance $CH = l$ from the center C to the chord. Then all possible positions of the chord PQ are obtained



by letting a run from 0 to 2π, and l from 0 to r, the radius. All interesting positions are obtained by letting a run from 0 to 2π, and l from 0 to $\frac{1}{2}r\sqrt{3}$. Therefore, the probability is

$$p = \frac{\int_0^{\frac{1}{2}r\sqrt{3}} dl \int_0^{2\pi} da}{\int_0^{r} dl \int_0^{2\pi} da} = \tfrac{1}{2}\sqrt{3}$$

If we take other variables, for instance, the angle a and the angle $b = \angle HQC$, then b runs from 0 to $\pi/2$ for all possible cases and from 0 to $\pi/3$ for all interesting cases and we get $p = 2/3$. These answers—and we can get any answer between 0 and 1 we wish—are both correct for their choice of variables. In terms of the theory of probability we may say that we have defined

"equally likely" cases in different ways, and in this formal theory all these ways are correct.

A definition which embraces the finite and the infinite cases of different character must use the definition of measure in the Borel-Lebesgue sense. We then have the following definition:

If a set of events of measure M is possible and we are interested in a subset of measure m, then the probability of this subset is

$$p = \frac{m}{M}.$$

The measure of a set is expressible as a summation in the Lebesgue sense. The formal theory of probability is therefore a chapter in the theory of Lebesgue summation.

A simple example in point set theory is this: the points of a line segment have coördinates from 0 to 1. What is the probability of hitting a point with rational coördinate? Here p is zero.

In this case we have assumed that all points are equally likely, otherwise we could not be sure that $M = 1$ and $m = 0$. Giving different weights to the possible and the favorable events means forming integrals on these point sets; the required probability is then the quotient of the two Lebesgue integrals.[1]

2. *The Experimental Facts.* This formal theory of probability has no special foundation-difficulty. It is as well founded as the different branches of mathematics which it uses for its computations: theory of numbers, theory of point sets, Lebesgue integration.

This theory bears some similarity to a series of experimental facts, and it is this similarity which makes it important to a larger group of people than that interested in permutations or point sets. To this similarity it owes its existence and name. These facts all deal with the regularity of the occurrence of certain special events in a long series of more general events. This regularity is, however, not a precise regularity; it is a rather loose type so that exceptions may very well occur and are even to be expected. Some of these experimental facts are the following:

[1] A clear statement of this aspect of the theory of probability is given by A. Lomnicki, FUNDAMENTA MATHEMATICA (1923), pp. 34–71.

a. In a large series of throws of a die, say N throws, each special number will be thrown a certain number of times, $n_1, n_2, n_3, n_4, n_5, n_6$. The quotients n_1/N, n_2/N, $--$, n_6/N will not change very much when N increases. If the die is an ordinary "honest" die, the quotients do not differ much from 1/6.

b. In a country, at a certain time, there will be always about the same percentage of people being born, dying and dying at a certain age during a certain period.

c. The number of automobile accidents in a certain state in a certain number of years will behave in such a way as to allow forecasting.

d. In manufacturing certain standard commodities (with specific faults) there will always be about the same percentage of commodities with those faults.

e. Many other well known cases may be added, dealing with the height of soldiers, the amount of rainfall, the number of telephone calls, the crime ratio, the molecules of a gas, etc. In all these cases we know nothing of the individual item except that it belongs to a series of possible events; we do not know to what case it belongs. Nevertheless, there is always a certain estimate which may be made about the cases when they are repeated. This estimate is accurate enough to allow betting with considerable possibility of success, as in lotteries, life insurance or engineering forecasts. To deal with these affairs the theory of probabilities has been invented.

It is usually said that facts like those mentioned depend on "accident." This means that we do not know in each case what event will happen, but we do not even care, insofar as we are concerned with the final result. The cases are selected in such a way that regularity results in the repetition of the events.

This is not a typical characteristic of "accidental" cases. Wars have recurred frequently enough but we can say little about their future outcome. Wars are "accidental," for we know very little as to when and where they will happen. It is the same with many social events such as bank failures, elections, stock prices, etc. There is seldom sufficient regularity in these cases to warrant any prediction by probability considerations. We can give

reasonable predictions on the number of automobile accidents in Massachusetts during 1933 by computations based on previous results. But such predictions would not lead very far if used on the presidential elections of 1936. We do not assert that it is not possible to forecast some of the results of this election; the forecast, however, will have to use other means than the theory of probabilities.

This reasoning will aid in answering this "paradox." We are not able to forecast the result of a lottery if we know all the factors that determine the game (dynamics of each individual drawing, psychology of the participants, etc.). The mathematics involved is too difficult. But, fortunately, we ignore most of these facts. Now the miracle happens that this enables us to make a very satisfactory prediction.

The answer is that the lottery is a very special type of accidental event, a type that follows simple laws by varied repetition. It is a series of events, each complicated, but asymptotically simple. The mathematics of functions behaving in somewhat the same way is familiar to us. For instance, the function *sin tan tan* $1/x$ behaves curiously enough near $x = 0$ but for large values of x it behaves like $1/x$.

The task of the theory of probability is to study what types of events will permit treatment by the methods of the formal theory and the reasons why such treatment is possible.

3. "*Equally Likely.*" One important difference between the formal theory and the experimental facts is at once obvious. In the formal theory a problem may have many answers according to what we call "equally likely." In the experiment the answer is unique. D'Alembert's problem of finding the probability of a tail in two throws of a coin has only one answer (with "honest" coins), namely, $\frac{3}{4}$. Bertrand's problem of the chords in the circle has, if tried with a stick which is thrown on a hoop, the answer $\frac{1}{2}\sqrt{3}$. The material obeys a law which determines the cases that are equally likely.

The question now arises as to what method we can use to find out what cases are equally likely. This question has never been answered in a satisfactory way. The answer will make the theory

of probability a science with the same security in its foundations as electrodynamics or mechanics.

As long as an accurate answer cannot be given we must use some heuristic principle which will furnish us with a lead. This is the value of the "principle of insufficient reasoning" expressed in its simplest form by Laplace. He states:

"La théorie des hasards consiste à reduire tous les évènements du même genre à un certain nombre de cas également possibles, c'est-à-dire, tels que nous soyons également indécis sur leur existence; et à determiner le nombre de cas favorables à l'évènement dont on cherche la probabilité."[2]

The principle states that we have to consider cases as equally likely when we have no reason to give one case more weight than another. It is a curious example of an argument by which we try to increase our knowledge by the simple method of having no especial knowledge at all. This may have weight in theological discussion where one may be able to convince himself of the existence of a Deity by summing up the many things he does not know. In natural science, however, the argument is hardly convincing. As Spinoza said, "Ignorantia non est argumentum."

The argument, notwithstanding its handicaps, has convincing significance in the classical die and coin problems, where it really can persuade us, if we wish, that the chance of throwing 1 with an honest die is $\frac{1}{6}$ and the answer to D'Alembert's problem is $\frac{3}{4}$. In more complicated problems it deserts us. Already in Bertrand's problem we are at a loss. We have given two solutions to this problem, one with a and l as variables and one with a and b as variables. It does not seem obvious that we should select a and l as variables instead of a and b because only the a and l have the property that we are just as ignorant of what happens between $a = 20°$ and $30°$, $l = 0.5r$ and $0.6r$, as of what happens between $a = 35°$ and $45°$, $l = 0.1r$ and $0.2r$. A similar example is the needle problem of Buffon. In a plane parallel lines are drawn at distance a. A line segment of length $l < a$ is placed in an arbitrary way on the plane. What is the probability that the line segment will intersect one of the parallel lines? The answer

[2] *Essai philosophique sur les probabilités*, 5me ed., Paris, 1825, p. 7.

is obviously just as indeterminate as that of Bertrand's problem; it depends on the variables introduced to determine the position of the line with respect to the parallel lines. But if we state the problem as the "needle" problem in which a needle is thrown on a table, then the two variables at once become uniquely defined. They are the distance of the center of the needle to one of the parallel lines and the angle the needle makes with this line. The answer is now $2l/a\pi$, which is justified by experiments.

A problem due to Tschebychef also offers some illumination on this subject. What is the probability of picking out, arbitrarily, two numbers among the natural numbers which are relatively prime?[3] (i.e., 7 and 13, 8 and 35). We can imagine all natural numbers written on cards of equal size and similar material; the cards are thrown into an urn and mixed. (We may limit the number to a large finite one.) One card is taken out at a time and not replaced, but there should be such a large number of cards in the urn that an individual drawing does not affect the probability of the next drawing. If we solve this problem considering each card as likely as another, the answer is $6/\pi^2$. A result obtained by Dirichlet,[4] without the theory of probability, corresponds to this answer. He considered the problem as one in number theory and asked: what is the distribution of relative prime pairs among the natural numbers? The answer is again $6/\pi^2$. That the answers coincide can be taken as proof that we were justified in considering all numbers as equally likely. This is, therefore, the only case so far considered in this paper where the choice of what is equally likely can be controlled in a rigorous mathematical way. Laplace's heuristic principle states that there is no number more likely than another. The justification lies in the actual computation by the theory of numbers.

The weakness of the principle of insufficient reasoning shows that there must be compelling reasons which are to determine equally likely cases. The principle is called indeed that of com-

[3] See eg. A. Fisher, THE MATHEMATICAL THEORY OF PROBABILITIES, 2nd ed., Macmillan, N. Y. p. 46.

[4] Dirichlet, *Werke II*, p. 60–64. See also E. Bessel-Ha e, ZAHLENTHEORIE, in Pascal's Repertorium, 2te Auflage, I, 3 (1929), p. 1518.

pelling or cogent reasoning and its validity can hardly be contested, but the way in which the principle has been carried out is very unsatisfactory. Von Kries, for instance,[5] has utilized a complicated logical analysis of the conditions which the different cases have to satisfy in order to be equally likely. In an actual problem this does not lead very far. Why, for instance, are all faces of an honest die equally likely? Certainly not because the die has the shape of a perfect cube alone, but perfect homogeneity of the material seems sufficient. It may very well be too much. Is equal situation of the centers of gravity enough? Or do we need second moments and must the ellipsoids of inertia also be congruent? Perhaps we must have equality even of third and fourth moments. The same objection holds in the case of the needle problem. We are sure that there must be cogent reasons for the superiority of the variables a and l, but what are they? The literature is singularly silent about these exact conditions. The problem of the foundations of probability will be solved when these cogent conditions can be given.

4. *The Frequency Theory.* These difficulties in the connection between the formal theory based upon the theory of measure and the experimental results have led many mathematicians to reject this mode of attack altogether and to begin entirely anew. This new attempt also has its opening chapters in a purely mathematical theory with no more difficulty in its foundation than any other branch of mathematics. This is the so called frequency theory which, at present, has R. von Mises as its principal defender.[6] The mathematical theory takes its simplest form in the following definition of probability:

Given, an infinite series of similar events, $e_1, e_2, e_3, e_4, - - -, e_k, - - -$, *each having one of the characteristics of a finite set* $w_1, w_2, w_3, - - -$

[5] J. Von Kries, Die Prinzipien der Wahrscheinlichkeitsrechnung. Freiburg, 1886, p. 11. "Die Aufstellung der gleichmöglichen Fälle muss eine in zwingender Weise und ohne jede Willkür sich ergebende sein."

[6] R. von Mises. Vorlesungen aus der Gebiet der Angewandten Mathematik, I. Band. Wahrscheinlichkeitsrechnung, F. Deuticke, Leipzig u. Wien, 1931. See also E. Kamke. Einführung in die Wahrscheinlich keitsrechnung. Hirzel, Leipzig, 1932, and J. L. Coolidge, Mathematical theory of probability, Oxford University Press.

$w_k, --$. *Let n be the number of events of characteristic* w_i *occurring in N events,* $e_1, e_2, ---, e_N$. *Then, if*

$$\lim_{N \to \infty} \frac{n}{N}$$

exists, this limit is called the probability of an event w_i *in the series e.*

According to this definition, the probability of an even number in the series of all rational numbers is $\frac{1}{2}$. The probability of a set of relatively prime numbers in the same series is $6/\pi^2$.

By passing to more series of events, definitions of total and compound probability can be given as sums and products of limits. Transition to geometrical probabilities is performed by letting the events depend on continuously varying coördinates. For series of events which have the limit property with respect to certain characteristics we get a rigorous theory of probability.

The connection with experimental facts is now obtained by assuming that these facts behave like the mathematical events of the definition. In throwing a die we have a series of throws $e_1, e_2, e_3, ---, e_k, --$, the characteristics $w_1, w_2, --, w_6$ being the six possibilities of throwing 1, 2, 3, 4, 5, 6. Now we assume that

$$\lim_{N \to \infty} \frac{n}{N} = p$$

exists, and this is the required probability.

There is a difficulty here not met in the other set-up of the theory. We assume here that this limit exists though we know that it does not really exist in the ordinary sense of the word. The difference $|p - n/N|$ does not necessarily become smaller than any preassigned number ϵ when N increases indefinitely. This objection is met by the remark that experimental facts never check completely with the theory. A stone, thrown away, will follow the parabola of Newton's theory but approximately. A bridge built according to the laws of elasticity obeys these laws to so small an extent that the engineer always introduces safety factors into the final results of his computations. We therefore force our frequency theory of probability upon the facts

with the same freedom which passes unchallenged in other fields of science.

This argumentation can be accepted, as long as we have no better way to account for the particular limits of probability theory. We shall see that the well defined mathematical expressions which indicate the actual behavior of probability phenomena are, in the simple cases investigated, limits in the mean in phase space (see No. 6). As long as we are not in the possession of these expressions, we may as well turn to the mathematical theory which we have under control, and introduce as a first aid the ordinary limits of elementary calculus. We do not express all the essential features of the phenomenon, but at any rate some of them. Laplace says that the "probability" of a certain phenomenon after many repetitions approaches a certain definite (ordinary) limit (Bernoulli's theorem). The frequency theory says that the frequency of this phenomenon after many repetitions approaches this limit.[7] In the first case we have a word with a vague sense and a rigorous limit. In the second case we have a well defined conception but the limit approached is not rigorously expressed by the limit we substitute for it.

It seems also that the difficulty of discriminating between equally likely cases is just as serious in this theory as in the theory of Laplace. The weight to be attached to each event in the series $e_1, e_2, --, e_n, --$ depends on assumptions in the mathematical cases, and it has to be found by actual experiment in the experimental cases. In d'Alembert's problem we must finally leave to the experiment the decision between all possible answers; we get 3/4 when we take head and tail as equally likely. Von Mises states this point of view clearly in his discussion of the needle problem.[8]

Bernoulli's theorem is simply a mathematical deduction from

[7] Kamke opens his book with an example which shows the weakness of the point of view of the frequency theory. He gives a series of values of the quotient n/N computed from a dice experiment. When computed to one decimal more these values oscillate in such a way that no exact limit seems to be indicated.

[8] L. c. p. 82. The kind of function to use for equally likely cases "kann nur durch das *Experiment* entschieden werden, und hängt, natürlich, von der Art wie die Würfe ausgeführt werden, wesentlich ab."

Laplace's theorem, and it does not add any information to the foundation question. We can easily put it aside.

5. *The Dynamics.* The cardinal problem is, therefore, to find out what causes the equal likeliness of events is nature or human practice. The line segment distributed haphazardly in the plane with parallel lines does not behave according to definite probability laws. The needle falling haphazardly on the table does follow definite laws.

The reason is that the needle differs from the line segment in that it obeys not only geometrical laws but also dynamical laws. When the needle falls and hits the table it behaves according to the Newtonian laws and the laws of collision. In the problem of Bertrand, actually checked with rigid bodies, a stick being thrown in the air to fall on a circle in a physical plane, we have an object subjected to the laws of the dynamics of rigid bodies.

It must, therefore, be in the dynamics of rigid bodies that a principle can be found which will allow us to discriminate between equally likely cases in the needle and the Bertrand problem, and in many other problems where rigid bodies are used, as in throwing dice. The task is in the simplest cases to find a criterion for equally likely cases by the use of Hamilton's laws:

$$\frac{dp_i}{dt} = \frac{\partial H}{\partial q_i}; \frac{dq_i}{dt} = -\frac{\partial H}{\partial p_i},$$

H being the Hamiltonian function $H(p_1, ---, p_n, q_1, --, q_n)$. These equations are the simplest form into which the equations of Newton can be cast; q_i are the generalized coördinates, p_i the generalized velocities of a system of n degrees of freedom.

The study of Hamilton's equations can best be performed in phase space of $2n$ dimensions with coördinates p_i and q_i. We thus come to the Boltzmann-Gibbs idea of considering the trajectories of dynamical systems in phase space. These trajectories behave in a way such that in the long run a certain geometrical configuration arises. This configuration determines the equally likely cases.

The fact that there exists statistical regularity shows that in phase space computation of equally likely cases can be performed.

The difficulty of carrying out the computations has been felt in statistical mechanics since Boltzmann's time—a mechanics which deals with relatively simple probability cases. It was for this reason that the ergodic principle was introduced to put forward the order which exists in the long run. The difficulty attaching to this principle has long been recognized,[9] and the proof, at or any rate, one correct formulation dates from very recent times. (See 6.)

In phase space the behavior of a dynamical system must, therefore, determine a measure which enables us to find out the relative probability of different cases. We have already found that the formal theory of probability is a measure theory. Formal and physical theory lead to the same conclusion.

A result of our theory seems to be (though only actual computation can justify it) that the faces of a cubical die are equally likely if the center of gravity of the die lies in the point of intersection of the diagonals and if the ellipsoid of inertia is a perfect sphere.

The conclusion is that a probability problem has a definite sense only if the dynamics governing the system under discussion is known. No probability without causality.

The dynamics behind a probability phenomenon need not be the dynamics of rigid bodies. In card games the dynamics must be differently defined. But when, in a certain period and a certain state, the number of "favorable" events is fairly constant, we know that there is a definite set of causes working in the same way during the whole period; these causes may be analyzed and even brought into the form of a dynamics similar to Newtonian dynamics.

If the causes change considerably, that is, if the dynamics is changed, another probability phenomenon arises. In this way we may expect a considerable change in the number of automobile accidents in a state when there is a goodly change in the number of automobiles on the road or when a new law is enacted. A war may change equally likely cases in the mortality of a country.

[9] See eg. P. & T. Ehrenfest, MECANIQUE STATISTIQUE, Encycl. d. sc. mathématiques, V, 2 (1912) p. 211.

6. *The Ergodic Theorem.* The progress reached in the last year in the statement of the ergodic principle can be summarized as follows: We consider a conservative dynamical system with Hamiltonian function $H(p_i, q_i)$. The motion of the system can be represented by the motion of a point P in phase space. After a lapse of time t a point will be carried from an initial position P into a new position P_t. The transformation which carries P into P_t carries phase space into itself and preserves volume in this space. If we now take a measurable point set A, it can be proved that under certain conditions the mean time in which the wandering point crosses A approaches a definite limit for increasing time, apart from a point set of zero measure. In symbols,

$$\lim_{T=\infty} \frac{1}{T} \int_0^T f(P_t)dt$$

exists in the above mentioned sense, if $f(P)$ is 1 on A and 0 outside A.[10]

This is the first case in which the ergodic principle has been exactly formulated. It shows how a dynamical system, left free, defines a probability density which can be taken as a Lebesgue measure in phase space. The great advance is that here the loose limits of the statistical experiments are replaced by exactly defined limits in phase space, but limits that hold only for zero measure. This explains why, in statistics, exceptions are possible.[11]

Collisions do not enter into the system under consideration. In die and other problems we have, however, to deal at all times with collisions. The question arises if the collisions alone are sufficient to determine a probability measure in phase space. A perfectly elastic needle should behave differently from a perfectly

[10] The theorem is due to Birkhoff, Proc. Nat. Acad. Science, 17 (1931), 656–660. A simple proof by E. Hopf, same Proc. 18 (1932), 93–100. The theorem also holds for more general functions f (Pt). I am indebted to my collegues E. Hopf and N. Wiener for many stimulating remarks on the subject of probability.

[11] The necessity of the application of Lebesque integration to a probability problem in physics is already shown by N. Wiener's investigations on the Brownian movement, Proc. Nat. Ac. Sc. 6 (1920) p. 253–260.

inelastic one. On dissipative systems no exact ergodic theorems are known.

We can also express our results in this way: In phenomena with statistical regularity a mixture of cases is performed. This mixture is thorough if the statistical regularity is sufficient. In the problem for which we gave the answer the laws of dynamics alone provide, in the long run, this thorough mixture. In the needle problem it may be the repetition of collisions which produces the mixture, if at any rate the experiment is performed by dropping the needle, not by pushing it in the plane. Different forms of the experiment give the same mixture. In many other cases we have to try out mechanical means of providing such mixtures. There are indeed stirring machines in use, for instance in cement mixing, which perform just the type of averaging which we need in statistics.

In the proof of the ergodic theorem we use the laws of dynamics, but we do not use all the properties. The only one we use is the group property of the transformations. It shows that the dynamics which causes statistical regularity is not a specific one but a type which is common to many dynamics. This is also borne out by considerations concerning the character of a probability phenomenon. In mortality cases in which life insurance is involved, not every factor which causes the death of the insured is of importance. Whether the person in the automobile accident died from a broken leg, broken hip or broken neck is not essential, no more than the make of the car. But there is a more general type of causality behind the mortality in which a company is really interested. The proof lies in the care with which big companies classify the general cause of death, as pneumonia, heart attack, accident, etc. There is, thus, the prospect that we can change the statistics by influencing essential causes.

This tends to show that there are stages of causality, and only certain general stages influence statistical regularity. If more causes appear, the statistical regularity may disappear. We can always influence a die so that it will fall on the 1. The ultimate stage is here a precise causality which produces rigorous regularity as in the case of the planets or the transmission of sound by telephone.

When we believe that we can use in a problem, the facts of which we do not entirely control, certain probability considerations, we express the hope that there are causal effects at work, but effects of a general nature that so far defy precision. If such causality does not exist there is no statistical regularity. We cannot predict the outcome of the civil law suits of 1934. A related instance is the "probabilité des jugements," on which the French materialists of the 18th century and the mathematical school with which they associated spent so much time. It was believed that there existed a kind of "dynamics of justice" underlying judgments, based upon the eternal principles of humanity, and as immutable as the principles of Newtonian dynamics. At present we do not believe in such a dynamics, and accordingly we have lost our faith in the applicability of probability considerations to judgment.[12]

Even in mathematics similar cases occur. Nobody knows the distribution of figures in the decimal development of π.

7. *Some Conclusions.* It is certainly not a new observation that there are no accidental events without causality. The idea has been expressed by Hegel (Logic, Book II, Section III, Actuality) and, since his time, has been repeatedly explained. As a fundamental principle in the theory of probability it does not seem to have been accepted.

Statistical phenomena cannot be understood without a definite dynamics. This does not mean that statistical considerations should be abandoned and replaced by dynamical considerations. The field of statistical applications remains as wide as it was. In some cases we may be able to compute the probabilities by a mathematical computation from the given dynamical laws, but these cases will always be few. The dynamics is often complicated, insufficiently known or hidden. In such cases considerations of a more hypothetical nature about equally likely cases must be used, as in insurance.

We can never speak of accidental events in the sense that there is no causal relation. When I call the presence of a dog in my room accidental, I do not mean that there are no special reasons for that dog being in that place. The presence of the dog is

[12] See eg. Laplace's "Essay" cit. footnote (2).

causally determined in every detail. The meaning of the word "accidental" in this connection is that the presence of the dog in my room is not causally connected with a specific series of events which I consider more general, or more important.[13] We say that a man died accidentally in an automobile collision and we mean that the general laws governing the man's life (his position, his mental make up, etc.) did not necessarily lead to this collision. One of the ways to show that these "accidents" really are subjected to as rigorous a causality as any other phenomenon lies in the statistical regularity of their occurrence. In this regularity certain small causes are enabled to reveal themselves.

This large effect of small causes in the long run is one of the fundamental remarks of Poincare[14] in his analysis of the theory of probabilities. He offers the example of the roulette, where small initial deviations produce important changes in the large. Also, small deviations in the orbits of the planets result in large differences in the final orbits. A small deviation in the initial position of a die that is being thrown causes different results to appear. This principle may enable us to define more closely the types of causality laws which produce statistical regularity.

In life insurance the statistical regularity is due to certain general causes working in each individual. This does not mean that the specific reasons which cause the death of a single individual are of any importance. For life insurance statistics these specific causes are inessential. The same is true for all statistical phenomena. They depend on causality but the causes that produce the regularity are not identical with the specific causes which produce each single occurrence.

Statistical and causal consideration of phenomena are, therefore, two different aspects of nature, each with its own field of application, and each justifying the other's existence. Without causality there is no statistics. But statistics can give us no more than statistical causality.

In modern physics it has become apparent that the laws we find for atomic phenomena are statistical. This is an interesting

[13] F. Engels, Dialektik der Natur, Marx-Engels Archiv II, 1927, p. 264.
[14] H. Poincaré, Calcul des Probabilités, 2e éd. Paris, 1912, pp. 4–20.

fact and appears quite plausible if we think of the large number of particles with which atomic physics deals. This should, however, not be interpreted as a demonstration that causality has to be abandoned in the explanation of nature. There is no absolute discrimination between statistical and causal phenomena. When we find that the physics of the atom is statistical in nature we state merely that the specific causes which rule the atom are indifferent to us, just as the life insurance companies do not analyze the death of each single individual though they know it is causally determined. It is, therefore, not necessary to believe that we can solve the difficulties of modern atomic statistics only by trying to construct a dynamics of the singular atom. But, on the other hand, atomic statistics can have meaning only if some general type of dynamics lies at its base and justifies its equally likely cases. Certain general aspects of this dynamics are already implied in atomic statistics, in the same way as certain general aspects of Newtonian dynamics are implied in the needle and dice problems. The carrying out of this program seems to allow the removal of many of the philosophical difficulties which have been raised by the statement that the laws of the atom are statistical.

The conclusion, sometimes derived from the statistical character of atomic phenomena as we measure them, namely, that all laws of physics are statistical and therefore loose or, at any rate, only partially valid, is not correct. If we start from statistical laws we cannot expect anything but statistical causality. But statistical laws do not exist without dynamics. Both aspects are equally valid. We may try to obtain Newton's law as a result of calculations in atomic statistics applied to macroscopic phenomena. This law of Newton will be a statistical law, but it will not be the only aspect of nature. There still remains the strictly causal law. If, in the needle problem, I compute the probability that the needle takes any position, I get 1 as my result. But the probability 1 in continuous probability does not mean certainty but only statistical certainty. Probability theory alone can never teach me that it is certain that the needle takes any position.

APPENDIX

I. MEASURE

The theory of measure deals with an extension of the conception of length, area and volume to sets of points. Such *sets of points* are collections of a finite or infinite number of well-defined points on a line, a curve, in a plane, etc. which may or may not lie within a certain interval. Examples are the sets of all points on a line of which the distance x to a point of a line is a positive integer, or a rational fraction. We also speak of "sets of numbers," "sets of spheres," etc.

A set of points may have a *limit-point*. A point $x = L$ on a line is a limit-point of the set S if there are points of S other than L in the interval $(L - \delta, L + \delta)$, however small δ may be. If we take, for example, the set given by $\frac{1}{2}, \frac{1}{4}, \frac{1}{8}, \frac{1}{16}, \frac{1}{32}$, etc., then $L = 0$ is a limit-point. Every set of infinite points in a finite interval has at least one limit-point.

If the set S contains its own limit-points, it is *closed*. An example is the set S of all points between 0 and 1, the points 0 and 1 included. If S does not contain its own limit-points, it is *open*. An example is the set S given by $\frac{1}{2}, \frac{1}{4}, \frac{1}{8}, \frac{1}{16}$, etc, because 0 does not belong to this set.

An infinite set of points is said to be *enumerable* if it is possible to define a one-to-one correspondence between the points of the set and the integers 1, 2, 3.... For instance, the set of points given by the numbers $1, \frac{1}{2}, \frac{1}{3}, \frac{1}{4}, \ldots$, is enumerable because we can associate to each number a definite place in the series 1, 2, 3.... But all points in the interval between 0 and 1 are not enumerable. The set of all rational fractions is enumerable because we can arrange them as follows: $\frac{1}{2}, \frac{1}{3}, \frac{2}{3}, \frac{1}{4}, \frac{3}{4}, \frac{1}{5}, \frac{2}{5}, \frac{3}{5}, \frac{4}{5}, \frac{1}{6}$, etc. The set of all numbers between 0 and 1 minus all rational fractions is not enumerable.

It can be proved that an open set, even if not itself enumerable, contains an enumerable set of non-overlapping intervals. We now define as the *measure* of the open set the sum of the lengths of its intervals. This sum is, as a rule, the sum of an infinite series, which always converges, as long as the open set itself lies in an interval of finite length. If such a measure can be defined in a unique way, we call the set measurable. The measures of measurable sets may be added and subtracted as ordinary lengths. If the point set is an ordinary interval of a line, the measure coincides with the length of the interval and the same holds for area, volume, etc.

There are point sets with an infinite number of points which have measure zero. All enumerable sets have measure zero, because we may

always include the points of this set in intervals of which the sum may be made smaller than any preassigned number. It is necessary only to include the first point in an interval of length ϵ, the second in an interval of length ϵ^2, etc., because the sum $\epsilon + \epsilon^2 + \epsilon^3 + \ldots = \epsilon/1 - \epsilon$ can be made as small as we like. The theorem cannot be inverted, because there are point sets of measure zero which are not enumerable.

We can also define a *measurable function* $f(x)$ in a given interval $a \leqq x \leqq b$. A bounded function $f(x)$ is measurable in this interval, if the set of points in this interval for which $f(x) > c$ is measurable for all values of c. Every continuous function is measurable.

Of a bounded measurable function $f(x)$ in the interval $a \leq \times \leq b$ we can now define the integral

$$\int_a^b f(x)dx$$

It can be done exactly like the ordinary integral. Let α and β be the lower and upper bounds of $f(x)$. Now take numbers $y_0, y_1 \ldots, y_n$ such that

$$\alpha = y_0 < y_1 < y_2 < \ldots - \ldots < y_{n-1} < y_n = \beta$$

Let e_k be the set for which $y_k \leq f(x) \leq y_{k+1}$ and e_n the set where $f(x) = \beta$. All sets e_k are measurable. Let their measure be $m(e_k)$. If

$$s_1 = \sum_{k=0}^{n} y_k \, m(e_k)$$

and

$$s_2 = \sum_{k=0}^{n} y_{k+1} \, m(e_k)$$

$$(y_{n+1} = \beta)$$

have the same limit with increasing n, we say that this limit is the *integral* of $f(x)$. This is the *Lebesgue* integral. For a continuous bounded function this integral is the ordinary integral.

For further details we refer to book on real functions, e.g. *E. C. Titchmarch*, "The theory of functions," Oxford, Clarendon Press, 1932.

II. VON SMOLUCHOWSKI'S THEORY

Only after the writing of this paper the author found out that some of its essential ideas have been clearly exposed by the late Professor M.

von Smoluchowski of Cracau, in a contribution to the Planck-Festschrift of 1918.[15] There we find the statement that the accidental is a special kind of causality relations: "Man nennt *Zufall* eine spezielle Art von Kausalrelationen," and the characterization of such relations by the Poincaré condition that small causes have large results: "Man sagt nämlich gewöhnlich, dasz ein Ereignis y vom Zufall abhängt, wenn es eine solche Funktion einer veränderlichen. . . . Ursache oder Teilbedingung x ist, dasz *sein Eintreten oder Nichteintreten von einer sehr kleinen Änderung des x abhängt* ("klein" im Verhältnis zum Schwankungs bereich des x)."

Mass. Institute of Technology,
February 28, 1933.

[15] M. von Smoluchowski, Über den Begriff des Zufalls und den Ursprung der Wahrscheinlichkeitsgesetze in der Physik. "Die Naturwissenschaften" 6 (1918), pp. 253–263. Both the work of v. Smoluchowski and of Wiener (footnote 11) is related to the Brownian movement.

An Examination of the Quantum Theories. I

BY

WILLIAM MARIAS MALISOFF

REFERENCE to quantum theory has become quite the fashion, and to base an expectation on an already receding episode with relativity theory, a great philosophical assault of interpretation of quanta is brewing in many quarters. The author of this series of studies would be content if by a plain statement of "what is the case" at this stage he could stem the more quixotic efforts, or rather reveal their unreal character to the spectators who may be enmeshed in the spell cast over such adventures. It is constantly necessary to restate the physicists' claims plainly in their positive content—as Einstein himself did once for relativity theory—so that one may know what is no-man's land and what is everyman's land in the new territory. This in itself is largely but a hope since there is no unanimity as to all the facts, and, worse, unanimity is no guarantee of stability. Nevertheless, the painstaking revision that is constantly going on on the experimental front enables us to arrange at least the data on a well-ordered scale of reliability. It then becomes much easier to detect false props to otherwise attractive theories, to discriminate live matter from the débris of progress, and to speculate on future growth. For such reasons the present examination of the various quantum theories will proceed from a presentation of facts and theories to their appraisal and then to a proposal concerning the organization of the facts,

concepts and theories. The subject is very complex: There are several quantum theories, some of which are already referred to as "classical" although these themselves were anti-classical (anti-Newtonian); all have claimed success in their time; all have serious hiatuses, including the present. The author has chosen to hark back to some of the older issues before appraising the latest claims.

THE PLANCK FORMULATION

The first of the quantum theories was proposed by Max Planck in 1900 as a technical means of resolving certain difficulties arising in the theory of radiation. Radiant energy, such as light and heat, had been found to flow according to simple mathematical laws. Foremost among these is the Stefan-Boltzmann law (1879, 1884) which states that the total energy flowing from a "black" surface varies with the fourth power of the absolute temperature (Ordinary Centrigrade temperature plus 273.1°). A "black" body has been rigorously defined and examples of almost theoretical black bodies have been found for experimental purposes. There is a rough continuity of absorbing and reflecting surfaces ranging from white to black (in the technical sense) which is reassuring to those who would stop to inquire how close the ideal law is to actualities. It may be quite close. It is easy, however, to forget the reference to "black," as one may observe even in technical communications. The perfectly black body is strictly speaking a fiction. Another, Wien's radiation law (1894), supplements the Stefan-Boltzmann law by describing the distribution of this flowing energy in a spectrum—it is decomposable into various colors or wave-lengths—and announces that the maximum intensity of a black body radiator varies as the fifth power of the absolute temperature. The two laws are bound together by simple mathematics which is a source of mutual reinforcement. Indeed all would have been well with this pair if, for instance, only visible and ultraviolet light had been studied. Yet the laws do not hold for long heat waves and the still longer electrical waves. If someone had not been interested in piling verification upon verification this lack of verification would have escaped notice perhaps quite indefinitely. There was perplexity one.

On the other hand the situation for heat waves was adequately met by the Rayleigh-Jeans law, which in turn broke down where Wien's relation triumphed. In a way this principle leans on the Stefan-Boltzmann but also asserts that the intensity of radiation at any temperature must increase for the shortest wave length (such as light) and tends to become infinite for very short vibrations (X-rays, gamma rays). This is quite untrue. But the Rayleigh-Jeans law, thoroughly untrue as it is in the short wave range, has been built on what had always been considered ground rock, the famous principle of the *equipartition of energy*. This "classical" principle amounts to a formulation of nothing but the most fundamental principles of Newtonian mechanics applied to a molecular system. It states that for a system in equilibrium the total energy per molecule is proportional to the number of degrees of freedom of the system (and the absolute temperature). The number of degrees of freedom is merely the total number of terms required to define exactly (unambiguously) the state of a system at any instant. If we admit Newtonian mechanics and the laws of chance and consider an idealized system which can be approached in experience and make adequate provision for equilibrium, we admit the principle of the equipartition of energy.

In such conflicts everything is thrown under suspicion. The specifications given above offer many possibilities for departures in the interpretation of actual systems. The writer, for instance, believes that there is still a possibility for much ado on the questions of fixing the number of degrees of freedom, the very laws of chance themselves, the notion of equilibrium, etc. Planck, however, did the desperate thing, and simply threw the equipartition principle overboard, and, by implication, Newtonian mechanics itself with it. An embarrassing illogicality attached itself to this act in view of the further fact that Newtonian mechanics was nevertheless to be retained where it suited. One was to continue to use a doctrine one of whose principal consequences was denied.

Some measure of smooth sailing was ensured by Planck by the "ad hoc" assumption that atoms oscillate or vibrate in a more or less simple periodic manner and that the atomic (or molecular) vibrators cannot take up energy continuously but do so dis-

continuously in integral multiples of a unit "quantum" of energy. Furthermore, this unit quantum, absorbed or emitted by the vibrator or oscillator, is proportional to the frequency of vibration. The equation of proportionality involves a constant which is the famous h, Planck's constant of action (*not* energy). To non-physicists it must be pointed out that this means that there are as many unit quanta as there are frequencies of vibration. It follows that if we have a continuous series of vibrators whose frequencies range from zero to infinity, we can still have a *continuous* series of quanta whose energies will range from zero to infinity. In view of this, exaggeration of the Planck assumption into assertions that it is "a denial of continuity" or the like are ridiculous. It must be noted that Planck's position underwent an evolution in three stages. He spoke first merely of discontinuous emission; then of discontinuous emission along with possible continuous absorption, and finally of energy exchanges (a) between resonators and radiation continuous for emission and absorption and (b) energy exchange between resonators and free particles (molecules, ions, electrons) in *quanta*.

It appears on closer examination that it is not necessary really to postulate any particular species of oscillators as Planck did, but that the discussion can move as well about the radiation itself, which inherits then the issue of the degrees of freedom within itself and the sensitive issue of the equipartition of these newly-conceived degrees of freedom. It would be idle to carp about the assumption of a particular kind of oscillator and to go on multiplying rival mechanical contraptions, a tendency which was immediately manifested in rival proposals. *Electromagnetic theory* (classical) *allows us to discuss the radiation itself regardless of the special mechanical construct back of it.*[1] The Planck assumption may be restated with regard to the radiation itself resulting in a glorified but clarified difficulty.

This demonstration of the irrelevance of the particular nature of the models leads the author to conclude that the difficulty or problem with which the Planck assumption attempts to deal is that of *transfer*, of transfer of energy from a (any) vibrating

[1] cf. J. Rice, Trans. Farad. Soc. II, Pt. 1. p. 1. (1915).

system to the medium in which it is immersed, and vice versa. The assertions of discontinuity, hence, refer only to the mode of transfer. They are anti-Newtonian in that Newtonian mechanics leads to expectations of a distinctly contradictory nature which are not verified for heat waves. According to Newtonian transfer schemes, which work with precision in the case of gross mechanical particles in motion, there must come a final state which is a degradation to the *shortest* vibrations of which the medium is capable. Thus tuning forks, for example, lose their energy to the air by a degradation to its natural frequencies. But in the Planck version of the fate of atomic oscillation it is the Newtonian degradation which is side-stepped.

If the question of transfer is seen to be the heart of the problem it becomes easy to see in turn why the "mechanics" of a quantum theory seem bizarre or impossible, but are still "mechanics." We have had no direct, convincing perceptions of such exchanges. The cleverest illustrations—what hasn't been invoked?—slot machines, change-making and what-not,—leave us cold. We are forced to speak of other varieties of the mechanical, conceivable but not demonstrable to the hopelessly prejudiced eye. In other words, in spite of the difficulty of model construction, we do not see the entry of any novel factor, such as a vital force, to explain the delay and the metering of the emission and absorption of energy, simply because we are dealing with the transfer itself as a physical phenomenon and are not tied down to oscillators, slot-machines, change-making entities, houses of cards, and the like. The failure of mechanical models saves us from vitalistic models as well by actually enabling us to locate the mechanical in the phenomenon of transfer itself. In such a light it is easier to follow the numerous controversies which have infested this subject.

PROPOSED IMPROVEMENTS ON THE PLANCK FORMULATION

An effort to get around the bald contradictions of Planck was made by Einstein[2] who produced Planck's formula by modifying the Maxwell-Boltzmann distribution law (for energy). This law

[2] Ber. deut. phys. Ges. 18, 318 (1916); Phys. Zeit. 18, 121 (1917).

allows the energy per atom or molecule to vary *continuously* from zero to infinity. The modification postulates that the atom or molecule is capable of existing in only a series of *discrete* states for which there are corresponding discrete energies. The absorption or emission of energy is merely the passage between any two such states. (Such states are what Bohr had called "stationary states" in 1913.) A few assumptions were made concerning these transitions, and these led by a little mathematics to Planck's relation! The most debatable or rather debated, assumption made by Einstein was that the reaction whereby energy was absorbed from the environment can reverse with a liberation of the energy to the environment. This, if taken without qualifications, opposes the classical law of the conservation of energy. And it was taken without qualification! A surprising willingness was shown by some physicists to accept the "law" of conservation of energy only as a statistical truth and "inapplicable in detail." Aside from the fact that the expression "in detail" meant apparently the consideration of an "open" system in which conservation was never supposed to apply by the most classical clasicist, Einstein's assumption did not need any such evasions. Reactions can reverse and do reverse under changed conditions which, following the fashion, may be traced to some collision mechanism or the like. What concerns us more closely in the discussion at this point is that Einstein did not rescue Planck from inconsistency. The quantum modification of the Maxwell-Boltzmann distribution, it seems, does precisely the same thing as the throwing overboard of the equipartition principle. It introduces the delay and the metering somewhere else in the "transfer" which is as oddly "mechanical" in the Planck way.

The efforts to derive Planck's relation with less and less reliance on classical dynamics are still continuing. Bose[3] (1924) produced a more polished effort than Einstein in the sense only that unlike him he leaned not at all on the Wien and Rayleigh-Jeans laws, but made use of statistical mechanics only combined with the supposition—now well verified—that the energy of radiation consists of quanta having momenta proportional to the frequency.

[3] Bose, Zt. Phys. 26, 178 (1924).

G. N. Lewis[4] (1930) gives a derivation avoiding the use of Einstein emission coefficients. What seems to be happening in these newer proofs is that the equipartition principle and classical considerations are being discarded *implicitly* rather than *explicitly*. Thus, the experimental findings on which Bose leans are inconsistent with the equipartition principle and are consistent with Planck's assumptions. The whole question is begged, since the aim of a "derivation" is to lead to the experimental results as a consequence, and not to assume them. One may well maintain that such proofs have not rescued Planck from inconsistency. The "transfer" which is oddly "mechanical" is still there, now more arrogantly as a "fact" rather than as a supposition. The glossing procedure is plainly a hopeless one and a treacherous one. What is needed is a new, rigorous system of truly non-Newtonian mechanics, *explicitly* elaborated in postulates (or laws) or a reestablishment of nothing but Newtonian mechanics, but not an illicit smuggling in of the very paradoxes that are to be resolved.

Our next study will show that the same state of affairs has existed in connection with the Bohr theory of atomic structure and its correctives.

SUMMARIZING STATEMENT

1. The original Planck formulation of quantum theory presents both continuity and discontinuity requirements in an inconsistent form.

2. Efforts to resolve the Planck paradox as such have only led to a substitution of an implicit contradiction for an explicit one.

3. Further consideration is invited of the notions of degrees of freedom, laws of chance, equilibrium, and departure from ideal conditions (black body) in a reexamination of the partition principle.

4. "Mechanics" may be rendered independent of models of any kind and may be made to refer to the *phenomena of transfer itself*.

[4] Lewis, Phys. Rev. 35, 1533 (1930).

Quantum Mechanics as a Basis for Philosophy

BY

J. B. S. HALDANE, F.R.S.

BIOLOGISTS have as yet taken but little cognizance of the revolution in human thought which has been inaugurated by physicists in the last five years, and philosophers have stressed its negative rather than its positive side.

The new quantum mechanics may be thought of, with de Broglie and Schrödinger, as wave mechanics, with Heisenberg as matrix mechanics, or with Dirac as a mechanics based on a non-commutative algebra of observables. However it is formulated it appears that space-time, even as modified by relativistic notions, is an inadequate co-ordinate system for matter. Two types of uncertainty emerge. On the one hand no observation can determine at the same time the position of a particle and its momentum with complete accuracy. On the other we have no certain method of distinguishing two similar particles. The first form of the uncertainty principle introduces us to the idea of events which cannot be definitely located in space or time. They have only a certain probability density in space-time. Of course, the probability is almost all concentrated in a region which, for most of our practical purposes, is a point-event. The second form shows that the individuality of an electron, though a sound practical idea for many purposes, is no more absolute than the individuality of most lower organisms. When the electrons approach within distances of the order of an atomic diameter, their

individuality becomes meaningless. In general to a pair of electrons, say in two H atoms, we can assign a number which (in the Heisenberg type of interpretation) measures the probability that they will exchange places within a given time. This increases very rapidly as the electrons approach.

Doubtless this probability interpretation is a concession to our apparently incurable habit of thinking in terms of particles and space-time. This lack of individuality in like particles gives rise to observable effects. Thus α particles (He nuclei) when shot through matter are deflected when they pass near the nucleus of a gas atom. Rutherford based his theory of atomic structure on this fact. Now a general rule of deflection can be laid down which holds for hydrogen, oxygen, argon, and all other gases with one exception. The exception is helium. For there is no absolute distinction between the α particle and any He nucleus, and as a consequence special terms arise in the equations for their interaction due to the so-called interchange energy.

The philosophical importance of these facts is enormous. They would seem to amount to an experimental disproof of the extremer forms of nominalism, even if the medieval realists would have said:

"Non tali ausulio nec defensoribus istis."

The nominalist regards all classifications as a matter of linguistic convenience and no more. Now if we consider a class of objects such as tables or dogs we can label any given member so as to distinguish it from any other member. There is no very obvious reason why we should stop our classification at "dog," rather than at "pug" or at an individual "Ponto." But this is not apparently the case with electrons. We can say that the universal "electron" is embodied in N individuals, where N if not infinite, is probably about 10^{80}. But we cannot label any individual or attach any verifiable meaning to its individuality.

It has been claimed (by myself among others) that modern physics are a refutation of materialism. I do not think that this is true. They are a refutation of the Cartesian view of matter as definitely localized in space, but I should call this theory spatial-

ism rather than materialism. Plato was guilty of gross anthropomorphism when he said:

"Θεος ἀει γεωμετρει."

Space is a human creation, as Kant realized, and the problem of the one and the many is probably also an antinomy created by the human understanding. In the case of the many electrons this antinomy has now been resolved. Unfortunately the solution can at present only be stated in mathematical symbols. A solution in words is rendered impossible by the fact that grammar is less elastic than algebra. For example, the pronoun "this" should not be used of electrons. But some are more nearly individualized than others. (This statement, being verbal, is a paralogism). The solution might be described as radical materialism. The physicist sticks to matter, to hard facts. If the matter refuses to resolve itself without contradiction into particles localized in space, so much the worse for particles and space.

Putting the same ideas in a different terminology, that of dialectical materialism, our ideas about matter are subject to an internal contradiction, which shows itself in the uncertainty principle. This contradiction is now being resolved.

Turning to biology we find a number of facts which are hard to reconcile with materialism as ordinarily understood. A living organism is always, in greater or less degree, a self-repairing and self-reproducing system. Every biologist, however mechanistic, takes this fact for granted. This fundamental postulate appears to differentiate biology from the physical sciences. Many biologists—perhaps most—think that such facts will ultimately be explicable in terms of physics and chemistry. But they have not yet been so explained, and a large group of biologists doubt whether they are so explicable, while a vigorous minority proclaim that they are inexplicable.

This latter belief may be stated in three ways. Vitalists postulate intervention of a vital force in mechanical processes. Such a force is generally likened to the human will. This point of view is exemplified by the writings of MacDougall. Another group, often idealistic in their outlook, hold that the facts of biology dis-

prove mechanism, and that the mechanistic account is incorrect not only for living organisms, but for other material systems. Physics and chemistry will have to be restated in such a way as to take cognizance of the facts of biology, and later of psychology. This is the view taken by J. S. Haldane among others. Finally, it has been suggested that while the laws of physics are not violated in living organisms, life takes advantage of the uncertainty principle to make certain events more probable than they would otherwise have been. This is the opinion of Compton, and I understand that it meets with some support among other physicists.

There are many objections to crude vitalism. Very careful work has shown that within the limits of experimental error whole organisms and their parts obey the same thermodynamical laws as dead matter. The detailed analysis by biochemists of the events taking place within the cell is continuing successfully along physico-chemical lines, and shows no signs of meeting any obstacles except the extreme complexity of the systems studied. But in the study, both objective and introspective, of human conduct, we find some facts, for example, the fact of purpose, which at first sight appear to be entirely foreign to a materialistic interpretation, and to call for some form of animism. I shall return to this problem later. Meanwhile certain psychological data should give animists pause. I refer to the data on the behaviour of monozygotic twins brought up in as similar an environment as possible, and particularly to the facts collected by Lange.[1] Such a twin pair certainly have different minds. One is not conscious of the other's experience. But these minds have a very similar physico-chemical basis. The parallelisms in behaviour are so startling that little room is left for factors determining moral choice which cannot ultimately be stated in physico-chemical terms.

The theory that mind takes advantage of the uncertainty principle is only a form of vitalism. For the essence of the uncertainty principle is that certain events are equally likely to occur. If mind renders one event more probable than the other, the laws of physics are violated to that extent. Moreover evolution ap-

[1] "Crime as Destiny" (1931).

pears to have been an escape from the consequences of the uncertainty principle.

If bacteria are heated or poisoned with certain reagents, the number of survivors falls off exponentially. This is taken to mean that the life of the cell depends on a single unstable molecule, whose change involves its death. As the transformation of such a molecule involves the uncertainty principle, this principle plays a large part in the life of bacteria. But higher organisms, even protozoa, behave as if their life depended on a number of similar molecules. The uncertainty principle in this form plays a less important part in their lives. They are protected from it by the laws of statistics, just as are large material particles consisting of many molecules.

The organicist standpoint has, I think, more to be said for it. In almost all biological work we take for granted the existence of the organism as a whole, as a system capable within certain limits of self-regulation and self-repair. This is still so when we are explaining in physico-chemical terms some detail of that self-regulation. This postulate until quite recently differentiated biology from physics and chemistry. It might be explicable in physico-chemical terms, but it seemed to constitute a real difference in methodology.

Nevertheless analogous problems exist and have been partially solved in physics. An atom is a complex system which automatically repairs itself after the loss of one or more electrons. Physicists have refused to take refuge in the idea of "the atom as a whole." The fact that the behaviour of a hydrogen atom, as revealed by its spectrum, is much more complex than could be deduced from the known properties of free electrons and protons, held up physics for a generation. But the physicists refused to surrender to holism. And finally the electron was shown to possess properties of a hitherto unexpected complexity, which can be expressed, perhaps somewhat figuratively, in terms of wave mechanics. The "wave systems" associated with different electrons interact, but in a predictable manner. The success of predictions based on this interaction is shown in such striking discoveries as the existence of two isomeric forms of H_2. There is no suggestion of

the "emergence" of a character inexplicable in terms of the constituent units.

It is perfectly true that these units have been found to be more complex than was suspected. And in particular they are not restricted to one region of space. The de Broglie waves of an electron extend out indefinitely. Nevertheless the interactions of such "wave systems" are susceptible of mathematical analysis of the same type as that employed in ordinary physics.

It may be that to explain the organism as a whole we shall require to postulate more complex properties in electrons and other constituents than are at present recognized. But I hope to demonstrate that the existing quantum mechanics do serve to explain certain biological and psychological facts which remained relatively mysterious from the point of view of nineteenth century physics and chemistry.

Meanwhile let us examine what is meant by "wholeness" as applied to an atom or molecule. Its most essential feature is what in physical terms is called "degeneracy," that is to say, a loss of degrees of freedom. For example, a neon atom consists of a nucleus and ten electrons. If these are all separate, as they probably are at a sufficiently high temperature (many millions of degrees) their motions have 39 degrees of freedom, apart from spins. Each can have any velocity in 3 mutually perpendicular directions. But in the atom almost all this freedom is lost. Only 3 of the 39 degrees are maintained at ordinary temperatures. Less rigid and symmetrical molecules have more degrees of freedom. They can rotate and vibrate at low temperatures, but they never have the full number of degrees of freedom that their constituents would possess.

As an example of the way in which these ideas can be introduced into biology, take the notion of heredity in its simplest form. When a cell divides in two the two daughters generally resemble the parent closely, and neither can be identified with the parent more than the other. The science of genetics has shown that certain quite small intracellular units, the genes, have this same property of reproducing their like. Moreover, when a gene within a cell is altered, at least in certain ways, the

so altered gene reproduces. The gene is (at least generally) below the limit of microscopic vision, and very possibly no larger than certain protein molecules. Other evidence suggests that it can legitimately be regarded as a large molecule rather than a group of similar molecules. The problem then is as follows: How does a molecule reproduce its like? It cannot be supposed to swell up and burst, like a drop of liquid. We can only assume that it is copied. That is to say it is stretched out in a thin layer or chain, and a similar layer or chain laid down upon it from materials available in the nuclear sap. The process can be compared to crystallization. I have discussed it in greater detail elsewhere. So the new gene may be regarded as a copy, and the old gene as a model. That is so from the point of view of pre-quantum physics. But the modern physicist would take a different view. Provided the genes do not differ as regards isotopic atoms, there is no certainty which of them corresponds to the original. The uncertainty, expressible as a probability of interchange, gives rise to a certain term in their joint energy. At most we can only say that one of the two is the more likely to be the original. And this would be so however carefully the system was observed. The truth therefore appears to be intermediate between the crude biological idea that the gene has divided in two, and the crude chemical idea that it has been copied. Actually the exchange probability would appear to be very small, that is to say the chemical idea is much more nearly true.

Similarly it is possible in a heterozygous, i.e. hybrid organism to say with certainty whether a given gene in a gamete was contributed by the father or mother. Apparently the paternal and maternal genes are separated mechanically. But in a homozygote each parent contributes a like gene, and these may be "chemically identical" so that at zygotene or pachytene when they lie close together their exchange probability becomes appreciable. It is thus impossible to say with full certainty that a given gene in a gamete from a homozygote is derived from one parent rather than another. And this is still so when the gene in question is located by closely linked genes.

It thus appears that the introduction into biology of modern

physical ideas enables us to throw some light on the biological phenomenon of heredity. There is a sense in which two molecules can both be the heirs of a like molecule. There is also a sense in which two molecules can both be the parents of two like molecules. In each case quantum mechanics suggests a compromise between the biological idea of equal heredity, and the chemical idea of no heredity. The compromise is based on the fact that a pair of genes in close approximation, whether at once after gene reproduction, or when the genes from two different parents fuse, form a somewhat degenerate system.

I have dealt with this particular problem in some detail because in the field of genetics we have the most detailed information into the behaviour within the cell of "sub-vital units," as Stapledon calls them. But we can tentatively apply the same principles to many other problems. The bacteriophage and some other small viruses appear, as Muller first pointed out, to be related to their hosts in much the same way as genes. Some recent developments in immunology and enzyme chemistry suggest that ideas of the same nature are applicable here also. But perhaps the possible lines of attack on other biological problems involving the organism as a whole will appear more clearly if we consider the very remarkable type of wholeness exemplified by mind.

Mind is a phenomenon associated with the nervous system of certain organisms, particularly man. We each know one mind—our own—from inside. The apparent distinctions between mind and matter may partly be due to this difference in view-point. I need not recapitulate the various arguments of monistic philosophers, whether materialists or idealists, which tend to minimize their apparent qualitative differences. But there are also differences in the type of organization. Let us compare a human mind with a piece of dead matter. The mind exhibits the phenomenon of purpose. Its activities are related to future events, either substantially certain or problematic. Thus I went to New Hampshire in August 1932 justly confident that I should be in the zone of totality of a solar eclipse, and hoping—falsely as it turned out—that the corona would be visible. This future reference would appear to be the main, if not the only, biological

function of mind, which has given it a survival value, and been a necessary (though not sufficient) condition for its evolution.

Mental events cannot in general be *exactly* located in space. While vision appears to be localized in the occipital cortex, Sherrington has pointed out that the co-ordination of binocular vision is, in his own words, non-spatial in certain cases. It would perhaps be more accurate to say that it cannot be sharply localized in space. It certainly depends on events inside our own heads rather than in our great toes or kidneys. Still less is it possible to locate a general idea present to our minds in one particular cerebral area. The study of cerebral injuries in man makes this point clear. Localized injuries outside the sensory fields do not wipe out particular small groups of mental events, but rather weaken mental functions in a general way. Lashley finds exactly the same to be true for the rat, in so far as its mind can be inferred from its behaviour. There are a number of learnt habits for which no particular part of the cerebral cortex is essential. Just as experiment shows imperfect localization of mental events, so observation shows their continuity in the case of perceptions where discontinuity might be expected. Thus the field of vision is continuous, and not mosaic, as might be expected from the fact that it depends on the impulses coming to the occipital cortex through some half a million separate nerve fibres. The same phenomenon is still more strikingly shown in the case of touch. It is useless to dismiss this fact as an illusion. Illusion is a fact about the mind quite as typical as true perception—perhaps more so.

Localization of mental events in time is also incomplete. They can doubtless be located between material events so far distant as to allow a relatively complex conscious process to be interposed between stimulus and response,—that is to say within a time of the order of half a second. This is about the length of the specious present, which gives a rough limit to the accuracy of introspective timing. It is moreover abundantly clear that in general mental events fade in and fade out, and that it is extremely hard to say just when they begin and end.

The mind is in some sense a whole. Several events are simul-

taneously present to consciousness, and intellectual activity consists wholly or mainly in grasping their mutual relationship. Just as there is a unity within a specious present, so there is a unity in time shown in the phenomenon of memory. Both these unities are of course limited. Mental events appear to go on which are not adequately integrated at the time of their occurrence. And past consciousness may be wholly, and is always partly, lost to memory.

Finally, although sensation is continuous and not atomistic, we encounter a kind of atomism in mind when we study its highest function of thought. It is a fact that we can establish a satisfactory 1 : 1 correspondence between certain mental realities and material events such as spoken words, or objects such as marks on paper. These mental facts are most typically universals, but may include individuals as known. The relations between them are of a simple character, and are discussed in text books of logic and mathematics. Typical universals such as "2" or "fallacious" are unlocalized in space-time, and can be apprehended simultaneously by more than one mind, thus rendering discourse possible.

We must now see how far in the light of modern physics a materialistic account of these facts is possible. Very clearly we cannot give a full account. Such an account would demand, among other things, a vastly extended knowledge of cerebral physiology. There is nothing novel in finding that mental events are inexactly localized in space-time. So are all other events. But the scale of indeterminacy is very large, in fact of the order of ordinary measurements, such as 10 cm. or 0.1 sec. If mind is to be regarded as expressive of the wholeness of the body, or even of the brain, it should probably be thought of as a resonance phenomenon, in fact part of the wave-like aspect of things. In a degenerate system degrees of freedom are lost because certain periodic systems oscillate together instead of independently. This resonance gives rise to various observable phenomena. It is responsible for certain terms in the energy of a material system. As the resonators are removed from one another, the energy falls off very rapidly. If mind is a resonance phenomenon we do not as yet

know what the resonators are. They might conceivably be molecules on the one hand, or more probably whole cells or large parts of them on the other. For of course cells in the nervous system undergo periodic electric disturbances, and Lapicque's idea of isochronism does not perhaps differ essentially from resonance. In any case the amount of energy concerned in mind must be exceedingly small.

If mind is a resonance phenomenon it is at once clear why it cannot be definitely located, either in space or time, though it is obviously enough connected with definite events in a definite material structure. The smaller the mass (or energy) of a mental event, the greater may we expect its indeterminacy to be. Thus the continuous character of our sensory experience becomes intelligible. From the dialectical standpoint the contradictions involved in an attempt to locate resonance energy exactly in space-time are the basis of the wholeness and continuity of mind.

Purpose, in a rudimentary form, has been shown by Fowler and Gamow to exist in "lifeless" matter. No reference to the future appears in the behaviour of matter on a macroscopic scale. We can safely confine water in a cistern where it would have to rise an inch in order to fall over the side. It is no more likely to escape if, after rising an inch, it can fall a mile, than if it can only fall half an inch. (I am, of course, taking an unduly abstract view by ignoring isothermal distillation.) The water does not display anything approaching purposive behaviour. But electrons do so. To escape from a metal into a gas at a lower potential, an electron must pass through a potential barrier. It does not possess sufficient energy to do so according to the laws of pre-quantum physics. But this does not in fact prevent its escape. From the point of view of wave mechanics we can say that the electron's wave system leaks through the barrier until the probability that the electron will be found outside it becomes large. This is a reduction to mechanical terms of the electron's apparently purposive conduct, and enables us to predict this conduct on a statistical scale. In just the same way an alpha particle escapes from the nucleus of a radioactive atom. Its wave system permits

it to act with reference to the field of lower potential outside the nucleus, which it will enter after traversing the barrier.

It is suggested that man also has a "wave system" which enables him to act with reference to distant or future events, this system being his mind. Of course no more in man than in the alpha particle does the future actually determine the present. We make our plans for the future on the basis of data now available. But on the basis of these data we deliberately undergo temporary unpleasure or pain for the sake of greater future pleasure, to take a simple instance analogous to the behaviour of the alpha particle. The analogy appears to be close enough to make it unnecessary to postulate the emergence of purpose as a qualitatively new phenomenon associated with mind. The electron can penetrate its potential barrier because its wave system effectively extends beyond it. The wave systems of the individual water molecules do not extend beyond the cistern to a sufficient extent to allow them to jump out of it on an observable scale. Nor does a volume of liquid as a whole possess the requisite wave system. But a man, or even a mouse, does.

This at once raises the very interesting question how far the mind, regarded as a wave system, could be said actually to extend out into space so as to comprehend distant objects. There is certainly no formal objection to this view, since the de Broglie waves of any particle are supposed to be omnipresent. There would seem to be at least a possibility of overcoming the objections to a realistic doctrine of perception which Berkeley and his followers have found in the physiology of the sense organs. In some sense the mind may actually extend out to its physical objects. When I perceive a spiral nebula some millions of years in the past there is perhaps a physical sense in which this nebula (separated from my retina by an interval of measure zero in space-time) is actually part of my mind.

The reality of universals is not repugnant to modern physics. As we saw, the electron is a universal embodied in a very large number of particulars, which show their manyness by obeying the exclusion principle, and their oneness by possessing interchange (or resonance) energy. Similarly, with a possibility of ultimate

physical consequences, we can say that the universal present in several minds is the same. It is less clear that the same relation holds between a universal as qualifying an object, and as forming a constituent of a mind which apprehends that object. The argument of the last paragraph suggests that this is not impossible, in which case a simple physical theory of truth would seem to follow. However, as I do not see how the necessary correlative theory of error would be formulated, I hesitate to put this view forward.

A universal apprehended by an individual mind, regarded as a wave system of exceedingly low energy, would have a high interchange probability, and a high uncertainty of localization in space-time. The apprehension by a mind of such a universal can be regarded with Whitehead as the ingression of an eternal object. But so can the synthesis of a new type of organic molecule. Plato's world of timeless unlocalized ideas represents a limiting or asymptotic case which would only be true for objects of zero mass and energy, just as the material world of pre-quantum physics represents the limiting case which would be true for objects of infinite mass. The truth lies between these two limiting cases, but it may well be that just as Newton's theory applies adequately enough for everyday purposes to massive bodies such as a dime or the moon, so does Plato's to its very exiguous objects.

But this theory, though adequate for most of the practical purposes of logic, is hardly adequate for philosophy. The universals as apprehended by our minds are not completely unlocalized and massless. If they were so they could not form part of mind considered as a physical system. They are not identical with the universals of Plato's ideal world. They are so nearly immaterial that, to a first approximation, their relations with one another are like those of Platonic ideas. This renders thought possible. But just because the units of our thought, the logical atoms, are not quite massless, they are infected with time and finitude. They cannot altogether

"Preserve their lips from tasting night and day."

Thought is never pure thought. It is my thought now. Because it is a physical phenomenon it is not infallible. For the same reason it can act on material systems. The mind of a timeless god is an asymptote which finite minds never reach, and such a mind would not only be changeless but unrelated to the events of a changing world.

In clear thought the Platonic ideas are so little distorted, so to say, that the theory that they are actually present as such to the mind is a useful approximation to the truth. But I do not think that it will bear ultimate analysis. We can perhaps obtain an analogy to the mechanism of their distortion by considering the much more highly localized universal "electron." The free electron, apart from all other bodies, is an abstraction. Its wave system is always distorted, and grossly so when it forms part of an atom. At any rate, the attempt to represent it, on relativistic lines, as substantially unchanged, has not yet succeeded. Should it succeed in future the co-ordinate system would leave it the same, but other universals, such as are involved in the idea of space, would be distorted. Similarly we think as best we can, but we need not suppose that our thought ever follows the laws of formal logic exactly, any more than matter exactly follows the laws of Newtonian physics.

There may or may not be a lower limit to the mass of logical units, and hence an upper limit to the perfection of mental processes. This mass would tend to diminish as a number of resonators increased. A mind associated with a sufficiently large and well organized "brain" might include events whose temporal and spatial indeterminacy effectively embraced all space, and all time also if, as seems possible, there was a first event and will be a last event in every material system. Such a mind could justly be described as divine, though certainly not as absolute. The material substratum or "brain" of such a mind might be a restricted portion of the universe, probably in the future, in which case its achievement is the goal of life. Or it might possibly be the universe as a whole, though this does not appear to possess the type of organization which we associate with the material substratum of mind. Nor does it possess the ethical perfection which

we should expect on such a hypothesis. But if the material basis of such a mind came into being in the future, it might, as Renan suggested, possibly come to know the minds of the past from inside, thus achieving their resurrection.

In any case, the present, past, or future existence of such minds is an empirical, not a metaphysical question. For example, the question of the existence or otherwise of a "world-soul" might be formulated as follows. First, is the universe as a whole a more degenerate system than was to be expected on current physical theory? Second, if this is the case, does the wave-like aspect of this degeneracy resemble that associated with mind? I know of no evidence suggesting a positive answer to these questions.

The following problem, however, arises immediately. If we identify the human mind with some part of the wave-like aspect of the brain, what are we to say of the wave-like aspect of matter in general? I doubt if we can identify it with any Hegelian absolute. I should prefer to think of it rather on Bergson's lines, as crude being, of which the mind is a specialized portion, more or less inadequately cut off and limited by the brain. This is, of course, a metaphor. But if we regard the wave-like aspect of matter as more fundamental than the particle aspect, we can surmise that it is of the same general character as human experience, but less, or at any rate differently, organized.

The idealistic account of the world starts with mind. On such a monistic view as I have put forward an idealistic account of the universe may be ultimately possible. But our knowledge about matter is a great deal more exact than our knowledge about mind, and less liable to be distorted by pride and prejudice. Moreover we have discovered in the uncertainty principle the internal contradiction in the properties of matter which makes it impossible to believe in the old-fashioned mechanistic materialism. As a matter of fact mind is subject to spatial, temporal, and other contingency. But philosophers tend to regard this fact as an illusion, partly on religious and economic ground, partly from a desire to have some foundation for theory which is not contingent. The physicists have repressed this latter desire fairly successfully.

The internal contradiction in matter as an interpretation of the

universe which, as I have tried to show, allows a materialistic account of life and mind, is the uncertainty principle. I do not think that the analysis of mind has yet been close enough to disclose its internal contradiction in any detail. Unless or until this can be done it is unlikely that any form of idealism will be as fruitful a philosophy as dialectical materialism. It can also be argued, on orthodox Hegelian lines, that the historical evolution of mind in material systems has actually been an overcoming of internal contradictions, and that the converse process did not occur historically, and cannot be exhibited dialectically. If the account of mind here given is in any way correct it would seem to be at least premature to attempt such a dialectic, and any such attempt could only be made by one to whom the wave-like aspect of matter is more familiar than it is to myself. I am inclined, however, to think that if we knew enough it might ultimately be as possible to give an account of matter in terms of mind as conversely. Such an account might occasionally be useful, as for example line co-ordinates are sometimes more useful than point co-ordinates in geometry.

We can at least point out the nature of the internal contradiction which would be the start of such a dialectical process. Idealists generally suppose that minds are immortal (which is not easy to disprove) and find it hard to explain why they are not also omnipresent, omnipotent, and omniscient (which they are clearly not). Some indeed, such as Sankara Acharya, have held that this is the case, and the finiteness of minds is an illusion. If mind possessed these properties, it would be a more easily intelligible system than it actually is. In the same way matter would be more intelligible if it actually possessed those properties which physicists until recently attributed to it. In truth mind is permeated with internal contradiction at every level. We can reason about eternity, but we are born and die. We measure in kiloparsecs and live in centimeters. We aspire and fail, we think and fall into error, we love and hate at once. A truly dialectical idealism would not meet these facts with remorse or denial, but with frank acceptance. So far, however, the idealists have not (in general at least) recognized that their ideas about mind are

incorrect. The mind as conceived by the idealist, therefore, remains a suitable locus for a static system of Platonic ideas. It has no place in a world where things happen. But once the mind's finitude is admitted, one can see in a very vague way how matter may be exhibited as a condition of that finitude.

Ethically also the program of a materialistic philosophy, provided it transcends materialism, is not only truer but more useful than that of idealism. In our own individual lives, and very possibly in our social development, we started with crude ideas appropriate to individuals as isolated as the atoms postulated by nineteenth century physicists. Such moral ideas might be appropriate to a castrated tom cat, but hardly to an animal capable of parental or sexual affection, let alone a social being like a dog or man. We realize the unsatisfactory character of these ideas, and transcend them to a greater or less extent. Dialectical materialism should and can help us in the process. It enables us to see in detail how, for example, the idea of personal property, while appropriate enough to a toothbrush, breaks down hopelessly when applied to a railway or a wife (in the U. S. A. a husband). Of course, the majority of dialectical materialists go much further than this, and adopt the ethical views of Lenin in detail.

An attempt to build up an ethical system from the other end has several disadvantages. It attempts to present the system as ready made, when it is really constantly growing, and growing by a dialectical process. Moreover once we admit that mind as such (or even man as such) implies a system of duties, we are confronted by a series of kindly but firm gentlemen who are ready to enunciate such systems. They offer us the Torah, the Bible (as interpreted by some particular church) the Uncreated Koran, and so on. It may be that in Russia Lenin's works are assuming the place of such a system. But at least they make no claim to finality, and explicitly point beyond themselves.

If such organisms as man possess a peculiar associated resonance system, the mind, it is probable that some system of this kind is associated with all organisms. It may be extremely rudimentary, but it is always an expression of the co-operation of like constitu-

ents. If, as I have suggested elsewhere, the origin of life is to be found in large organic molecules which could, by a process analogous to crystallization, reproduce their like by building up similar molecules from simpler constituents, then such a system would only be alive at the moment of reproduction. Life, as a continuous process, would only begin when a number of such molecules were more or less permanently associated. We must be prepared for the idea that bacteria which break up and pass through a filterable stage actually cease to be alive in doing so. When the filterable materials are again built up into bacteria life perhaps begins anew. Life then would be a resonance phenomenon between molecules. One of its most obvious expressions is in morphological patterns which possess a certain stability.

It gives the organism a certain bias in the direction of wholeness. It is important to realize how slight this bias is in many cases. Most of the higher plants and many of the metazoa can be sub-divided to a considerable extent without killing them. Their cells cannot be so divided. The cell is a far more fundamental unit of life than the whole organism, as we should expect if life is a phenomenon of resonance between molecular resonators rather than larger structures.

If mind is a resonance phenomenon, not between molecules, but between larger aggregates such as cells or parts of them which undergo periodic electrical disturbances whose periods are of the order of a thousandth of a second, there is a very wide gap between mind and crude life considered as physical systems.

This gap may well be bridged to some extent by other types of being. There may be something corresponding to the "animal soul" between mind and the low-grade life which shows itself in mere assimilation. We can at present form no physical picture of such connecting links for several reasons. We are beginning to get a fairly clear picture of molecules, and to measure accurately, though by roundabout methods, the frequencies associated with them. The frequencies of oscillatory disturbances in the nervous system are so low as to be directly measureable. Moreover, we know mind from inside. Living organisms may well include oscillators of intermediate size, with intermediate frequencies.

Nevertheless, there is no reason to attribute mind to such organisms as protozoa or higher plants, and it is probably at best exceedingly rudimentary in all but a few animals. In so far as different minds agree in apprehending the same universals, it would seem that they do actually form a physical system with certain properties not found in a single mind. Such a system, or community, is not a mind. It may be the crude embryo of what Alexander calls deity. But the properties of such a system, in fact the ethics of a community, are not necessarily incapable of incorporation into the physics of a very distant future.

The dialectic of egoism and altruism is familiar. In so far as the individual knowingly acts as a member of a society he negates himself. But if it is a highly organized society and not a mere mob, in so doing he actually gains in concreteness by playing a unique part in that society. Quite similar phenomena are familiar in physics. An electron as part of a material system loses its identity in greater or less degree. In a degenerate gas it achieves no compensatory gain in concreteness. In a molecule it does so, becoming, for example, one of the bonding electrons between a pair of atoms.

A few words may perhaps be said as to the possibilities of life-like and mind-like "emergents" associated with material structures other than those of the living organisms known to us. Such may exist in connection with a plant or animal species as a whole, owing to resonance between the lives (considered as physical systems) of the individuals. But because these lives show mere similarity and little co-ordination into a system, such beings, if they exist, would probably possess few and simple properties. Only one species, our own, shows any beginnings of co-operation between all its individuals, and that only within the last few decades, when most of them have been incorporated into a single economic nexus. And here matters are immensely complicated by the fact that solidarity occurs on the mental rather than the vital level.

We are just beginning to form ideas as to the state of matter in the interior of the stars. Most of the matter in white dwarf stars is in the state of degenerate gas. The electrons and nuclei

do not form atomic systems of the ordinary type, but are much more densely packed, and the great bulk of the star appears to behave in many respects like a single molecule or atom. Further, according to Milne, ordinary stars mostly or all contain cores of this same dense type of matter. The discovery of the neutron also opens up the possibility that dense types of matter of other sorts exist. Thus if there are particles with the mass of a proton but a unit negative charge, they might unite with protons or other positive nuclei to form a very dense type of matter unlikely to be found near the surface of heavenly bodies.

While the resonance phenomena within the main body of a white dwarf or the core of a star would probably be of the relatively simple type found in crystals, in the transitional region between the gaseous and degenerate part of a star relatively small aggregates of degenerate gas might exist capable of something analogous to the chemical reactions which occur at such a transitional layer as the earth's surface. It is not inconceivable that in such systems resonance phenomena of the complexity of life and mind might occur. It may be that the thermal conditions do not allow of the necessary stability. If any such stability is possible a very long period seems to be available for their evolution, and it is conceivable that the interior of stars may shelter minds vastly superior to our own, though presumably incapable of communication with us.

I am aware of the extremely speculative character of the opinions put forward in this paper. Any or all of them may be rendered ridiculous by the progress of physics or biology. But some residue may serve as a nucleus for more exact thought in the future. Very obviously if they have any truth the simpler constituents of matter are likely to have more complex properties than we yet attribute to them. But it is, I think, of importance that philosophers, and even ordinary persons, should realise that a thorough-going materialism is compatible with the view that mind has many of the essential properties attributed to it by metaphysicians. The theory here presented does not reduce it to an epiphenomenon of matter, but exhibits it as a reality interacting with ordinary material systems.

A biologist cannot hope to have achieved more than a most superficial acquaintance with either physics or metaphysics. I do not doubt that I have been guilty of elementary solecisms in my discussion of both. Nevertheless his somewhat central position may give him a certain assistance in discussing their relations to one another. They are related, not only through the fact that mind knows physical objects, but because mind is associated in another way with certain physical objects, namely material organisms. I have, of course, stressed this latter relationship to the almost complete exclusion of the former. This was inevitable in so short a presentation, and I can also plead that the philosophy of the last two centuries has left more room for novelty in a discussion of the theory of life than of the theory of knowledge.

Some of my statements must appear mutually contradictory, or even self-contradictory. This is inevitable in a discussion which accepts the Hegelian dialectic in any of its modifications. It is not my personal fault if the words which we use to describe matter and mind are based on internal contradictions. On the contrary, it is desirable that such contradictions should be exhibited. For it is only by an analysis of these paralogisms that we are likely to approach a unified view of experience. It has been my object to suggest that the progress of modern physics has made such a unified view more readily attainable than appeared likely ten years ago.

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Indeterminism and Natural Selection

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ABSTRACT

The historical origin and the experimental basis of the concept of physical determinism indicate that this basis was removed with the acceptance of the kinetic theory of matter, while its difficulties are increased by the admission that human nature, in its entirety, is a product of natural causation. An indeterministic view of causation has the advantages (a) of unifying the concept of natural law in different spheres of human experience and (b) of a greater generality, which precludes the acceptance of the special case of completely deterministic causation, so long as this is an unproved assumption. It is not inconsistent with the orderliness of the world, or with the fruitful pursuit of natural knowledge. It enriches rather than weakens the concept of of causation. It possesses definite advantages with respect to the one-sidedness of human memory, and to the phenomena of aiming and striving observable in man and other animals. Among biological theories it appears to be most completely in harmony with the theory of natural selection, which in its statistical nature resembles the second law of thermo-dynamics.

In an indeterministic world natural causation has a creative element, and science is interested in locating the original causes of effects of special interest, and not merely in pushing a chain of causation backwards *ad infinitum*. These contrasting tendencies are illustrated by a critique of the mutation theory, and by an attempt more closely to define the sense in which indeterministic causation should be thought of as creative.

THE ancient philosophers seem to have thought that with few or no observational facts, but guided solely by their intuition, and the power of exact deductive reasoning, it was possible to arrive at a valid and scientific view of the nature of the world around them. In some civilisations, science, or philosophy, seems never to have passed beyond this speculative phase, and in our own intellectual tradition there are many centuries in which speculation was pre-

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dominant, and the direct, inquisitive scrutiny of things, neither encouraged nor respected. In the last few centuries a certain uneasy compromise may be observed, between inventing the world *a priori*, and looking to see what it is like. The framework of our thoughts, our preliminary concepts, or basic ideas, have been supposed to be given by philosophy; it was the business of observational science to fit into this framework its missing details, in all confidence that whatever could really be found, had already its place prepared for it in our conceptual framework of ideas about what the world is like.

One of the most remarkable things about the science of our own generation is that, at one point after another, we are coming to recognise that the observations will not necessarily fit into their assigned places. What we had imagined must be, is found not to be so in truth. The limitations of the unguided human imagination are found not to be limitations of the surrounding world. We must let the facts sink into, and inform, ideas more deep-seated in our nature than we could have thought amenable to their influence. Of these examples of the dissolution of the axioms none perhaps carries more far-reaching consequences than the present questionable predicament of the doctrine of determinism. We may consider briefly the character of this predicament in its relation to the history of physical science, before turning to some parts of biological theory, in which the release from this doctrine, as from a dogmatic restraint, seems to open up fresh and fertile possibilities.

What has been called the "principle of indeterminacy" has been much discussed in recent years by writers on pure physics. We need not here discuss it, beyond noting that in spite of its name it does not, however true it may be, settle the problem. In the present state of knowledge it *may* merely define a necessary limitation of human powers of observation; but, again, it may imply that the exact specification of the state of a physical system, in sufficient detail for its subsequent behaviour to be inferred with exactitude, itself corresponds to no physical reality prior to this subsequent behaviour. We must take it as leaving the question open, though it, and the wave mechanics in which it appeared,

have sufficed to bring to the forefront the question: Why should determinism have ever been regarded as necessary, or even as a reasonable assumption?

We must go back to the seventeenth century for the formation of the existing concept of physical causation, which has governed scientific thought on the subject over the intervening period. Terrestrial and astronomical mechanics were given at that time a scientific basis and, what is equally important, a mathematical form. In the mathematical form adopted, the future state of a system could be calculated rigorously, supposing its initial state were known with absolute precision. The prediction did not involve a plurality of future possibilities, having determinate probabilities, although this mathematical form, appropriate to indeterministic predictions, had been developed in connection with games of chance, almost as early as had been the laws of mechanics. During the eighteenth century the methods of mathematical physics were found to be applicable not only to the mechanical properties of matter, but to those of heat, light and electricity; so that the simple notion of completely deterministic causation seemed to be applicable in the whole physical realm, and the task of science seemed to be merely to ascertain the form of the equations by which effects might be expressed in terms of their causes. Some uncertainty was felt, it is true, as to the extent of the physical domain. As late as Hertz, in the latter half of the nineteenth century, a cautious physicist might set living matter deliberately aside, in formulating the universal laws of mechanics. On the other hand Descartes, in the seventeenth century, had not hesitated to propose that all animals, with the sole exception of man, were mechanical automata without feeling or will; and at the beginning of the last century Laplace had found it necessary to conclude that the development of the solar system, including the whole course of human history, could have been computed by a sufficiently able calculator from the positions and motions of the particles of a primitive nebula.

The equanimity with which a deterministic theory was accepted as applicable to lifeless matter, and to organisms lower than man, was undoubtedly facilitated by the belief that the human race

was separated from the rest of creation by differences, not of degree but of kind, and of a kind which transcended in importance the differences which distinguish all other beings, whether living or not living. Man knew himself to be capable of reasoning, and felt himself to be capable of choice. He could, it appeared, act at will, and thereby to some extent control his surroundings. The fact that the physical sciences had so far stripped the non-human world of these attributes added much to the repugnance that was felt to admitting, as it was inevitable he should soon be forced to admit, that man himself was but an animal, descended from lower animals, and owing his evolution from them to the same great causes, which had brought the rest of the animal creation into existence.

The strength of the deterministic position did not only rest on its power of comprehending the most diverse physical phenomena, but on a much more insistent fact. As, in each branch of research accuracy was increased, as disturbing causes were eliminated or allowed for, as purer materials were used, and as more refined methods of measurements were discovered, so were the physical laws more and more exactly verified. The mathematical formulae were constantly found to be more accurate than the observations upon which they had first been based, or, when their form had to be modified, they were found to be so much the more comprehensive. It is not to be wondered at that experimenters and theorists alike should have regarded it as axiomatic that there *were* laws of causation, similar in form to those already established, which controlled the happenings of the world with perfect rigour. The argument was an extrapolation from experience. The physicist might say:—"By taking precautions against observational error I have reduced my errors to a tenth of those that used to be committed. By further refinements of the same kind, the errors that remain could be still further reduced, and so "*ad infinitum*." But, though the argument went further than experience can ever proceed, the simplicity of the axiom, and the insistence of the verification that the argument should certainly be carried further than experience had formerly proceeded, made its acceptance well-nigh universal.

How firmly rooted was this conviction may be judged from the fact that when its experimental basis was removed, the change was scarcely noticed. For the experimental basis fell away, not with the advent of the wave mechanics during the last few years, but a full half century earlier, with the acceptance of the kinetic theory of gases, and of the more general statistical mechanics, as a means of explaining the properties of matter. The experimenter who, up to that time, might have imagined that the precision of his instruments, and the exactitude of his observations could be increased indefinitely, by precautions of the same kind whereby they had reached their existing state of perfection, had now to recognise that this was only true so long as he did not approach, in geometrical magnitude, the size of a single molecule, and so long as the energy of a single molecule was negligible compared to the quantities he was measuring. On the other hand, the accurate predictability of the physical behaviour of systems involving much larger quantities than these, was guaranteed on statistical principles by the large number of independent particles, of the aggregate, or average, of the actions of which this behaviour was the outcome. There was nothing in this to prevent one believing what was, indeed, generally believed, that the behaviour of these individual particles was strictly determinate. But, equally, there was no experimental basis for believing it, and no reason for doing so except the continuity of habit. That determinism would be rediscovered behind the molecular chaos, was, though unrecognised, a new axiom, to which the reliability of physical law in larger masses—the basis of all previous experience of determinism —could, by its nature, give no support. Consequently the interest of the new physics, dealing with single electrons, and quanta of energy, is not that it has discovered an indeterminate system, as to which we must remain, for a time, in doubt; but that it has conspicuously failed to discover the deterministic laws (in the sense of the explicit formulation of future in terms of past experience) which were for long sought with unswerving confidence.

It is a revolution in theory only. Technically, since technique must always stick close to the facts, the difference it makes is trivial. If we take the view that all laws of natural causation are

essentially laws of probability, we are only recognising in theory what we have always known to be true in our daily affairs. Theoretically, it is of interest that the reliability or predictability of a system has the same basis now in the "Natural" as in the "Social" sciencies. We have the same reason to be confident that a physical system will behave as it should, as we have for predicting approximately the number of marriages which will take place next year in London—the reason that the result depends on the aggregate of a large number of independent actions, the average character of which has been determined by previous observation. And if a trade depression affecting millions simultaneously, should temporarily affect this average behaviour, just as the chilling of a beaker will retard a chemical reaction taking place within it, this only shows that the average is conditioned by the environment, and not that the argument from the approximate determinism of the aggregate to the exact determinism of the individual is any stronger in the one case than in the other.

Besides unifying the concepts of natural law held in diverse spheres of human experience the view that prediction of future from past observation must always involve uncertainty, and, when stated correctly, must always be a statement of probabilities, has the scientific advantage of being a more general theory of natural causation, of which complete determinism is a special case, possibly still correct for special types of prediction. Consequently the man of science who is determined to abide by Descartes' precept never to accept anything for true which he does not clearly know to be such, has no option, in the absence of cogent evidence for determinism, but to set this aside as an unproved assumption, which, in spite of its familiarity, is unnecessary to a rational and coherent approach to scientific studies.

Biologists, perhaps, as this view becomes more familiar, will be more inclined to elaborate independently concepts appropriate to their own field of study, and be less inclined to assume that all improvements in method must be those introduced from the "exact" sciences of physics and chemistry. They may also come to wonder what the controversy between "mechanism" and "vitalism" can have been all about. In sociology, as I had oc-

casion to illustrate last year in the Herbert Spencer lecture,[1] it may be of real importance to recognise that some links in the nexus of social causation are more rigorous, owing to their dependence on numerous independent actions, than others, which are influenced much by fluctuations in public opinion or in public policy, in which far fewer main agencies may be discerned.

The *appearance* of social determinism evinced in the history of the rise and decadence of many great civilisations, which has impressed, though not always convinced, the most philosophic of historians, would seem to be capable, on this view, of a new interpretation. It would appear that the political efforts of so many generations had expended themselves to so little real effect, merely because they have been usually devoted to pretentious futilities. To sweeping back, as it were, a rising tide with a broom instead of building a dyke, or to making the sun rise by crowing at the approach of dawn. Man, it would seem, has not yet laid hands on the levers which control his sociological destiny, but has instead exerted himself to retard or accelerate the belts and wheels by which existing causes are bringing about their necessary effects. The course of social evolution in the history of civilised man, gives the appearance of being controlled by necessary and unintelligent causation, merely because intelligence has not in fact yet been applied to its guidance. Things which are allowed to drift will naturally appear to be at the mercy of the current.

The author is conscious that it is probably, at the present time, premature to forecast the effect of an indeterministic view of the world upon biological opinion on the theory of evolution. We can note the framework of tendencies amidst which this reaction will be worked out, and bring into at least a preliminary perspective the majestic proportions which the concept of evolution itself assumes; and the status, from our new standpoint, of the different causative processes which have been invoked to explain it. The first point to be emphasized, perhaps, is that an indeterministic theory does not in the least imply an anarchy of causelessness. It implies only that certain limitations, once regarded as

[1] The social selection of human fertility. Herbert Spencer Lecture, 8 June 1932. Oxford: The Clarendon Press.

rigid and necessary, will appear now as conditioned or even as casual. The existence of order and harmony in the world, difficult to define as these conceptions are, is as much an observational fact on one theory of causation as on another; natural law is none the less real if, when precisely stated, it turns out to be a statement of probability: causation is none the less recognisable, and an action is just as much an effective cause of subsequent events, if it influences their respective probabilities, as if it predetermines some one of them to the exclusion of the others.

In connection with the effectiveness of causes there is a point of still greater importance which will be easily appreciated only by those who have entered unreservedly into the logical consequences of the deterministic position. To the complete determinist that action which the common man regards as a cause must itself be regarded as wholly predetermined. It could not have been different. And since to the common sense of mankind it is the property of a cause, *qua* cause, that it might have been different and have had different effects, the supposed cause, if completely predetermined, is recognised not to be a true cause at all, but must yield place to some prior event, of which it is itself one of the consequences. But the determinist cannot stop there, nor indeed anywhere within the realm within which determinism is assumed to be complete. If he ascribes to any act the full sense of causation, it can only be to the act of creation by which his deterministic universe was brought into existence. Within the history of this universe there is neither cause nor effect, but only a predetermined pattern of events which for some reason unexplained by deterministic theory is perceived by men as ordered in a time sequence with memory running in one direction only. Those known first are only conceived to be the causes of those which follow, through the illusion that they might have been different, in which case, logically impossible as it is, the deterministic scheme would, it is supposed, have required a modification of the subsequent events. It is obvious that there can be no observational basis for studying the consequences of what can never happen, and to the determinist, who perceives the logical consequences of his theory, causation is as non-existent as free-will. In other words

only in an indeterministic system has the notion of causation restored to it that creative element, that sense of bringing something to pass which otherwise would not have been, which is essential to its commonsense meaning.[2]

The fact that our consciousness has in what we call memory a mode of access to past experience which does not extend to experiences to come, is to the determinist not merely an inexplicably arbitrary limitation of man's outlook on the world picture, but one that is in positive contradiction of the view that the state of our consciousness is governed by the instantaneous condition of a physical system following a predetermined course. For the instantaneous state of such a system must be related to its subsequent states by equations identical with those which relate it to its previous states. That this is not the case with the human mind is, like the existence of the power of effective choice, universally verified by subjective experience. Unlike this latter belief, however, it is also directly demonstrable by objective experiment. One has only to make a series of random drawings from a pack of playing-cards, recording each card as it is drawn, and between each pair of drawings to allow the subject to record the best opinions he can form as to (a) the card last drawn, and (b) the card to be drawn next, to demonstrate expermentally that the opinion recorded under (a) correspond more closely to the facts than the opinions recorded under (b). It may be noted that the cogency of this demonstration would not be impaired even if the subject showed some degree of clairvoyant prediction, unless he could foresee the future correctly no less frequently than he could remember the past, as would be the case if the two series of events were connected with the instantaneous state of the consciousness by identical equations.

The creative element in the relation of cause and effect makes an essential difference to the view we take of the spectacle of indi-

[2] It is necessary to emphasize this aspect of causation since so revered a leader as Max Planck has referred to physicists who adopt the statistical method as "setting aside the principle of causation" (*'Where is Science Going'* p. 64). Clearly here the principle of determinism is intended. It would be more just to speak of the statistical formulation of physical laws as reinstating the principle of causation.

viduality among organisms, and of the associated phenomena of aiming and striving. It is possible to imagine that man is an automaton, and that the verb "to choose" exists in all languages only because he is an automaton predestined to the illusion that he has a power of choice. The biologist must then decide (or at least his nervous mechanism must go through motions which seem to him like deciding), when he sees apparently purposeful effort in other organisms, whether they also share the same illusion, or whether it is a peculiar product of the evolutionary process in the case of man. It would, indeed, be perfectly possible to argue that, since certain purposes connected with survival and reproduction have to be achieved by any organism which is to maintain its place in the world, a purely deterministic world would, in fact, evolve creatures which behaved as though their actions were purposive. But the *illusion* that they could act or refrain from acting spontaneously, since what really happens is not affected by it, can be of no selective advantage, and the appearance of this illusion in mankind must be postulated as a purely fortuitous accident; feelings analogous to those experienced by man in choice and intention must be denied, unless the accident be required to have occurred repeatedly, to all organisms not closely related to man.

An indeterministic world, then, is one in which the human qualities of aspiration, planning and foresight, are rationally possible and may be advantageous, and in which we can recognise the primitive precursors of these qualities, not as an epiphenomenon, but as having a real part to play in the survival or death of the organisms that evince them. Biologically it might be said that purposive action by the organism as a whole is the crowning stage of an evolutionary process by which relatively large masses of living matter have come to achieve that co-operation of parts and unity of structure which we call individuality. For, on a statistical view of causation, spontaneity or creative causation is at its highest only when perfect unity is achieved.

In the childhood of our race creation had to be regarded as a single catastrophic event. The imaginative faculties were insufficiently developed to conceive it as a continuing process still

active in the midst of its incredible duration. It was Darwin who first opened men's eyes to the marvel that causes already known were effective in the creative process of fashioning living things. His mind had set itself to ponder, in a rational spirit, on the adaptive characters of animals and plants, that is on those qualities which distinguish them most conspicuously from the non-living world; the adaptations of the internal structure, and the intricate physiological mechanisms by which the co-operative functioning of the whole body is made possible; the complex interaction of growing parts by which the different structures of the body are formed; the external appearance, and morphological adaptations to exploit or escape from other organisms; and finally the nervous mechanism which gives them instinctive guidance, emotions and incipient reason.

The only other serious attempt which has been made to explain these qualities of living things, either before Darwin's time or since, had been made some 60 years earlier by the speculative zoologist Lamarck. Lamarck had suggested that the desires and efforts of animals had the power of inducing in them heritable changes which should enable, by an imperceptibly gradual process, corresponding desires and efforts in their descendants to be more perfectly satisfied. In the case of plants, where the driving force of will could not be (or at least was not) postulated, Lamarck fell back on the unsatisfactory expedient of ascribing their evolution to heritable changes induced by their environment. Lamarck had no satisfactory explanation to give as to how the appropriate hereditary changes were to be induced by the volition of animals ignorant, as most must be, of their own adaptive requirements and potentialities; and in the case of plants, as perhaps unaware of the existence of numerous and intricate adaptations to the habits and properties of other organisms, which these habits and properties could not possibly have induced. Darwin, from an early age, was familiar with too many facts incompatible with the Lamarckian hypothesis, to gain from it the least assistance in convincing himself of the reality of evolution; and he unquestionably found that the rejection, by biologists of the two previous generations, of Lamarck's uncritical speculations, had prejudiced

the case against all evolutionary doctrines. It is not that Darwin was in theory at all averse from the inheritance of acquired characters. On the contrary, as I have pointed out elsewhere[3] in more detail, on the theory of inheritance accepted by him, the environment ought constantly to be modifying the heritable constitution. It was only his unceasing and impartial study of observational facts that led him constantly back to the view that, as evolutionary agencies, such modifications had in fact achieved very little. How could the existence of hairy or woolly animals have induced the seeds of plants to grow hooks, fitted to be entangled in the hair, and facilitating the dispersion of the seeds? How could the instincts and special structures of worker bees have been evolved by a Lamarckian process, through a series of queens who never attempted the actions for which these instincts and structures are adapted?

It was, therefore, not until Darwin had satisfied himself that differences in the rates of death and reproduction would alone suffice to ensure a constant improvement of all adaptations, that he became a convinced evolutionist. This theory postulates nothing more than the existence of heritable variability, and need make no assumptions about the causes by which new variations (mutations) arise. It ensures, moreover, the improvement of adaptations of all kinds, throughout the entire life-history, by which the rates of death and reproduction may be beneficially modified.

Two characteristics of Darwin's evolutionary thought which, in the writer's opinion, give it its supreme and lasting value are, first, that while an active theorist, willing to follow long chains of reasoning if he felt their foundations secure, he constantly brought his speculations into contact with a candid and thoughtful scrutiny of the living things themselves; and, secondly, that he was never willing to curb or limit his thought by theoretical considerations brought in from other fields of study. Thus his theory of evolution made demands upon the age of the earth beyond what the physicists, for many years after his death, regarded as physically possible. We now know that the evolutionists were

[3] The Genetical Theory of Natural Selection. Oxford: The Clarendon Press, 1930.

right and that it was the physical theory that was faulty. Again, in regard to man and his mental and moral characteristics, (his Soul in all but the meaning given to the word by theological theory), Darwin did not shrink from the plain meaning of the facts that fighting cocks could be bred for courage, or dogs for fidelity, and that the amazing plumage and ornaments of many male birds indicated a discriminative choice on the part of the females, which only an anthropocentric prejudice would hesitate to call aesthetic appreciation.

Both on the Lamarckian and on the Darwinian theories creative causation is centered in the organisms themselves. Each living thing has a part to play in the process; though few can achieve so much as that intrepid explorer, the first vertebrate who voluntarily left the water. The contribution of each is made effective, however, on the Lamarckian view by willing and striving only, on the Darwinian view by doing or dying. The selective value of choice and spontaneous action must always lie in its harmony with the world around, its capacity to utilise its advantages, or penetrate its undiscovered possibilities. An active animal above all requires to establish a sensory and emotional contact with all its dangers and opportunities. Conscious and more or less intelligent mental life would seem to have played an increasing part in the adaptation of the higher animals to their surroundings, and the diversity and intricate possibilities of these surroundings seems to have provided a necessary and sufficient condition for such mental development.

Darwin's selection theory is alone among the theories of evolutionary change in locating the driving force of the evolutionary process in this manifold contact between the inner and the outer worlds. It is alone, in fact, in placing no reliance on the manner in which hereditary changes—mutations—come to happen. About the causes of these mutations and about their nature, whether generally beneficial or predominantly deleterious the theory of selection need postulate nothing. It is sufficient that they should maintain some hereditary variability in structure and in behaviour, and that some forms of structure and some types of behaviour should be, in fact, more successful than others.

All alternative views of evolutionary causation have attempted to locate the driving force in the origin of hereditary variations, that is in the process of mutation itself. Thus, though the term "mutation" was not used in its modern sense at the time the Lamarckian theory was developed, that theory postulated that it would be sufficient to produce evolutionary change if the action of the organism in willing or striving could induce in itself the appropriate mutations. Again a considerable body of doctrine under the name of "Orthogenesis" postulates that a long sequence of progressive evolutionary changes may be brought about by purely internal causes producing a constant succession of mutations in a predetermined direction. The least common measure of all this group of theories, in the sense of postulating least, and explaining least, is the so-called mutation theory, held by some geneticists, which, when stripped of its earlier and more ambitious extravagances, seems to assert no more than that the process of mutation is the effective cause controlling the direction of evolutionary change.

Even this bare residuum of the many attempts which have been made to place the effective causes of organic progress in circumstances far removed from the vital activities and interests of the individual organisms, is in contradiction with a substantial body of well-ascertained fact. A knowledge of how hereditary factors are inherited makes it possible to calculate, with rough but sufficient accuracy, how rapidly fresh variation must be supplied by mutation, in order that the variability may be maintained. Under a particulate scheme of inheritance, such as is now known to prevail, this rate is many thousands of times smaller than under the blending theory of inheritance, which was assumed, prior to the rediscovery of Mendel's work. Until that rediscovery it was quantitatively reasonable to put forward hypotheses involving mutations occurring with sufficient abundance to cause, of themselves, an appreciable rate of evolutionary modification. On the Mendelian theory it has become several thousand times less reasonable to do so.

Moreover genetical workers, especially with the fruit fly *Drosophila* and with maize, have found it possible to examine suf-

ficient material to detect the occurrence of the more frequent and striking mutations, and to estimate the mutation rates with which they occur. This work indicates that, though some mutations, especially, perhaps, some of the lethals, may occur with mutation rates of more than 1 in 1,000,000 in each generation, yet that this is a relatively high rate of occurrence, and that mutation rates exceeding 1 in 100,000 are quite exceptional. It is easy to show that mutation rates of this order would be entirely inoperative, in causing evolutionary change, if they were opposed even by very minute counter-selection.

These are quantitative considerations: they can carry their full conviction only when the grounds of calculation have been examined and the relevance of the quantitative evidence assessed. This I have attempted to do elsewhere.[4] For the present purpose it is sufficient to notice that all theories which ascribe the effective guidance of the evolutionary process to the agencies which cause mutation are exposed to this quantitative difficulty, which has not yet, in any case, been resolved. We must ask the proponents of all forms of mutation theory, though we must at present ask in vain, whether they are willing to postulate mutation rates many thousands of times greater than such as are known to exist, and if so, whether they have an alternative theory of inheritance reconcilable with these hypothetical mutation rates. The difficulties of the mutation theory, however, are not only quantitative. The kinds of mutations which are observed to occur are equally difficult to reconcile with the view that these indicate the direction in which evolutionary change is proceeding, for of the many hundreds of mutations which have now been studied none can be claimed as definitely advantageous to the organism in its wild environment, and the great majority are definitely pathological deformities. Indeed the largest class of all comprises the so-called 'lethal' mutations, which render the organism incapable of living to maturity. If these mutations pointed the direction of evolutionary change there would be no escape from the conclusion that all the species so far sufficiently studied by geneticists were in a state of rapid degeneration. It may, indeed, be argued that

[4] The Genetical Theory of Natural Selection. Oxford: The Clarendon Press, 1930.

the mutations studied by geneticists are not a random sample, and still less an exhaustive catalogue, of the mutations which are capable of occurring, and which are occurring occasionally among the thousands of millions of individuals which constitute most species. The geneticist can only study conveniently mutations which have a relatively great and distinct effect on the external appearance. Again the mutations which are observed to occur must be, preferentially, those which are occurring with the highest frequency. For these reasons, the mutations upon which genetic experience is based may be expected to produce larger effects, and to have higher mutation rates, than many others which more frequently escape observation, or fail to occur in the stocks under examination. There may therefore, in addition to what has been studied, be a multitude of mutations having effects smaller than can readily be observed, and these if deleterious, will naturally on the whole be less deleterious than those whose larger effects are more easily studied. Some of these inconspicuous mutations, indeed, may not be wholly disadvantageous at all, but may be capable, in particular environmental circumstances, or possibly in particular genetic combinations, of conferring a benefit upon the mutant organism. To recognise this, however, is by no means to admit the possibility that the causes of mutations are the effective causes of evolutionary change; for if, in some cases, slight or rare mutational changes lead to a beneficial modification of the species in which they occur, such as other more drastic and more frequent mutations are incapable of doing, the determining cause must lie not in the production of mutations, but in the selective process, (the net result of the vital activities of the living population), which determines that the beneficial mutations shall be effective, and the deleterious mutations, more powerful and more numerous as they are, shall be inoperative as causes of evolutionary change.

There are, therefore, in the facts which have been so far observed, both as to the frequency and as to the nature of the mutations whose occurrence has been studied, obstacles to the acceptance of the mutation theory of evolution which there seems at present no prospect of being overcome. In studying or specu-

lating upon the causes of mutations we are paying attention to a process which apparently can exert no appreciable control over the evolutionary progress of organic nature; and this although the occurrence of mutations itself, like the physical conditions necessary to sustain life, is a necessary condition of future evolutionary change, and may be regarded as supplying the ingredients or raw materials which may be utilised in the fashioning of evolutionary modifications.

It is particularly worth noticing, in this connection, that it is only in a completely deterministic scheme of causation that the earlier links in the chain of events are causally equivalent to those that follow them. So that a Laplace could, by pressing logic to an extreme, regard the motions of particles of a primitive nebula as, in a real sense, the determining causes of consequences taking place to-day. On an indeterministic theory intermediate events cannot be neglected or eliminated. In consequence scientific research is interested not merely in the manner in which precedent events determine or influence their consequences, but in locating in time and place the creative causation to which effects of especial importance are to be ascribed. In the present condition of evolutionary theory there can be little doubt that it is in the interaction of organism and environment—in the myriad biographies of living things—that the effective causes of evolutionary change must be located.

In speaking of the location in time and space of the creative causation which is responsible for effects of special interest, it should be remembered that all that we mean by the designation creative is that certain happenings entail consequences, or entail systems of probability for various consequences, other than those that could have been foreseen from antecedent happenings. It is not the writer's intention that this term should imply any foresight or will directed towards these consequences; as would be the case if the creative element were due to the intervention of a personal Creator. The discussion of such intervention appears to be outside the province of natural science. The writer is discussing only the creative element inherent in any system of natural law which is specified in terms, not of certainty, but of probability.

Dealing with the matter after the fashion of mathematical statistics, we may distinguish in any event a creative, or casual, component defined as the deviation of the event in question, in its quantitative aspects, from the average to be expected, according to the laws of nature, from previous occurrences. Natural law is supposed to determine the probabilities of future events with exactitude, and therefore the average value, though not the direction, of the casual component. It is appropriate to speak of this component as casual in relation to preceding causes, and as creative only in relation to the consequences which it entails. The entire system of ideas might indeed be regarded as a simple extension to causation in general of concepts long familiar in the Darwinian theory of evolution by natural selection.

An examination of this parallelism, in relation to the foregoing critique of evolutionary theories, suggests yet another limitation, which may be borne in mind when we speak of the components of happenings which are not predetermined as 'creative'. Somewhat analogous to these in evolutionary theory are the mutations of genetics. (This of course is not to suggest that these mutations are undetermined, but only that the concepts are analogous.) The reader will remember that we have insisted with some emphasis that the process of mutation should not properly be regarded as a cause governing the direction of evolutionary change, but only as a condition which renders evolution possible. In the same way it is probable that in relation to any effect of special interest we should never think of the casual component of any one particular event as by itself creative, but should recognise the creative quality as belonging only to aggregates of such casual components.

This opinion, if well founded, is not probably to be taken as reflecting any objective property of the external world, but merely a common limitation of the kind of effects, as to the causes of which the human mind is prone to enquire. Such effects must, it seems, always present some element of simplicity which our minds can grasp. Thus we may ask intelligibly why a pebble is round, or what has made it round, whereas to specify in detail the geometrical configuration of pieces of matter of the more complicated

shapes that occur, and to ask the same question of them, would be a far less natural enquiry. The explanation that it has been rounded by the abrasive action of surf on a beach is a satisfactory one because we recognise that such action will be less destructive and more uniform on a round pebble than on one with edges or corners and not at all on account of our ascribing any special rounding tendency to the chipping or scratching caused by each particular impact. The whole process is thus a cause of roundness, though we should feel it unnatural to speak of any particular event of the process as such a cause. The simplicity of one aspect of an aggregate of results has stimulated enquiry in the manner in which I believe research is usually stimulated, and the enquiry has led us to recognise a multitude of causal happenings, the statistical aggregate of the effects of which affords us the explanation we require. It is in such cases proper to speak of the aggregate of causes, rather than of any particular element of it, as creative.

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DISCUSSION (CORRESPONDENCE)

In this section we shall publish brief communications commenting on published papers or bringing up special points of interest which can be discussed by correspondence.

ON THE APPLICATION OF MANY-VALUED SYSTEMS OF LOGIC TO PHYSICS

Sir:

In a recent Letter to the Physical Review Zwicky[1] proposed a "principle of flexibility of scientific truth," which he appears to regard as either a consequence of, or at least in keeping with, an application to physical reasoning of many valued systems of logic. Since the fallacy of his basic argument may not be entirely apparent to the casual reader, and the matter is one of very vital importance, I desire to outline, in this brief space, what I consider a pertinent criticism against the immediate invasion into physics of the radicalism implied in Zwicky's note.

I

The use of a many-valued system of logic does not lead to many-valued truths. A many-valued system of logic is a code of inference which endows propositions with truth-values intermediate between true and false; i.e. statements about the physical world are permitted, for instance, to be either certainly true, or possible, or doubtful, or certainly false. Give such a set of truth values, the mathematical logician sets about to combine propositions into functional forms, and to investigate the truth-values of the combinations relative to the truth-values of the propositions involved. Any combination which is always true, or validly assertable, regardless of the truth-values of the constituent individual propositions, is called a *law* of the logical systems. A functional relation whose validity depends on the nature of the propositions which it involves is, by definition, *not a law*. It is clear that each many-valued

[1] F. Zwicky, Phys. Rev. 43, 1031 (1933).

logical system in general leads to logical laws, and these laws are rigorously assertable, inflexible, one-valued.

Unless we change the conventional use of the term, physical laws, like logical laws, are propositions which are valid. But because of their empirical character, physical laws, unlike logical ones, are in a state of flux, and this fact has long been known to physicists. It implies, not that a law at a given time may be sometimes true and sometimes false, but that it may cease to be a law. This temporary validity of physical laws has its roots in the progressive method of collecting physical evidence and presents no intelligible relation to the choice of logics. To be sure, it is conceivable that by selecting a different system of inference physicists might be led, on the same basis of physical facts, to different, but equally definite laws. Again these laws would partake of the transitory character of physical truth, but they would be inflexible as long as they exist. If experience showed a many-valued logical system to be preferable to the two-valued one, and if this system led to different physical laws, we should say that our present laws are wrong and shift to the new but equally definite set of laws. One might claim in this connection that the possibility of an infinite number of logical systems all of which are equally "true" would lead to diffuse physical laws, and therefore flexible truth. However, this claim is easily invalidated by observing that different logical systems, producing different laws, cannot be applied jointly if the results are to be significant.[2]

II

Flexibility of scientific truth is incompatible with every system of logic so far proposed. Zwicky suggests that, apparently as a result of an application of many-valued logics, or something "deeper" than many-valued logic, one must question every categorical statement. As pointed out, many-valued logic admits of a series of truth values including the equivalents of true and false, and perhaps including doubtful. This means that every proposition is either true, or false, or doubtful. The scheme certainly admits true propositions. If it did not it would be useless, for it is easy to find any number of propositions which are certainly true. Indeed, by questioning uniformly every absolute statement one produces what might be termed a system of one-valued logic, which permits no reasoning at all.

[2] C. I. Lewis, The Monist 42, 481 (1932).

III

There can be no quarrel with any reasonable attempt of applying new logical systems to our present body of physical evidence. But this can not be done by *questioning* present laws and *guessing* at more general possibilities. These latter activities are the natural rights of any scientist; they derive their justification, not from logic, but from the status of physics as an empirical science. They constitute no new type of reasoning and are no *method* in any strict sense of the word. The only correct way of applying the new logics comprises three steps: 1) Development of a system of logical laws on the basis of assumed truth values. 2) Development of a system of mathematical formulae consistent with the logical laws. 3) Derivation of physical laws by the use of these formulae. In the course of this work it may turn out that arithmetical numbers are unsuited for physical analysis. Step 1) has been taken;[2] step 2) is being made in pure mathematics; an attempt at step 3) is unknown to me. It may be claimed, however, that it will lead to as many types of physical statements as there are truth values, and that among all the statements those which are both certain and significant will be selected as laws. These laws will constitute physical truth—if the latter term is to have any meaning, and such truth will not be flexible.

IV

The examples which Zwicky chooses to illustrate his supposed fundamental principle amount to denials of specific physical laws. They reflect, in part, a sound skepticism but bear no relation to many-value logics. I do not wish to burden this note with a detailed logical analysis of his interesting proposals except to remark briefly on the possibility of the suggested reversal of time. As regards the epistemological status of time, two extreme views can be taken: First, time is a concept which we have as an a priori datum somewhat in the sense of Kant's transcendental esthetics. In that case there can be no argument about a reversal of time, for we have no means of visualizing it by intuition. Second, our notion of time is due to statutory definition; in particular, physical time is a definite order imposed upon phenomena by reference to mechanical clocks. Then a reversal of time would either destroy this order, i.e. time itself, or be meaningless. (The more familiar concept of reversibility is not subject to any such criticism, of course, for it designates a reversed order of *phenomena in time* while the clock

itself runs forward.) But even if these arguments regarding the nature of time were wrong, logic alone could never be used properly to deduce a single quality of empirical time.

It seems altogether improbable that the difficulties to which Zwicky points can be resolved by altering the logical structure upon which our reasoning is based. Perhaps it is not useless to regard them from a somewhat different angle, in spite of the fact that the business of rejecting fundamental principles of reasoning in preference to revising existing theories has become fashionable. If an elementary particle exists in space and is not a point, then it must be capable of an infinity of variations and its proper description requires an infinity of variables. Classical theory and wave mechanics ascribe to the electron only three such variables. The success of Dirac's theory is largely associated with the introduction of a fourth quantum number, and it may be argued that its failures are due to the restriction to four numbers. In that case, what is required is not a new system of logic, but a new physical theory incorporating a higher degree of variability than the old.

H. Margenau.

REVIEWS AND NOTES

Dialektik der Natur, by Friedrich Engels. Herausgegeben von D. Rjasanov. Marx-Engels Archiv II (1927), pp. 117–395. Frankfurt a. M.

Modern natural science forces us to think more and more in terms of what Hegel called dialectics. The different conceptions of energy and impulse, of matter and wave, of space and time, appear all in their relative value, each having a special field of application, with a certain definite reality behind it, but never the full reality, and each under circumstances passing into the other. This development is a continuation on an extended scale of what began in the beginning of the nineteenth century. Before that time only two sciences were fully caught in its motion, namely mathematics and mechanics. The transition of algebra and elementary geometry into analytical geometry and the calculus, and the discovery of the identity of the laws controlling the fall of the apple and the motion of the planets are classical examples of a dialectical motion in scientific material. Up to the end of the eighteenth century, however, there was little more to physics, chemistry and biology than simple classification of isolated facts.

With the nineteenth century an entirely new outlook developed. Discovery after discovery tore down the barriers between the branches of knowledge, showing relationships, connections and even identities unknown to previous generations. It is mainly this development that Engels' "Dialektik der Natur" studies and analyzes.

Engels himself, in notes dating from 1873 to 1892, mentions especially three important discoveries of a dialectical nature. The first is the discovery of the cell structure in plants and animals by Schleiden and Schwann. The second is the principle of the conservation of energy relating motion to heat, heat to light, light to chemical energy, etc. The third is Darwin's theory of evolution, which, according to Engels, gave the biological supplement to the theory of human society which he and Marx had developed. The "Dialektik der Natur" gives many more examples of the same general laws of nature.

Friedrich Engels, who died in 1895, was the friend and collaborator of Karl Marx. He was not only an active politician but also a keen scholar in an astonishing number of fields. His notes on the philosophy of nature were for years under the custody of Edouard Bernstein at Berlin, who failed to realize their meaning. Einstein, who saw some of the manuscripts in 1924, advised in favor of publication "because they form an interesting contribution to the understanding of Engels' importance." Now the notes have been published under the able direction of the Russian scholar Rjasanov, who added an interesting introduction and a detailed index.

The book consists of short remarks and short essays on varied topics in mathematics, physics and chemistry, ranging from simple quotations and marginal remarks to highly interesting productive contributions (e.g. the "Alte Einleitung zur Naturdialektik," 1880, p. 239, or the remarks on the accidental and the necessary, p. 264). Engels, as a pupil of Hegel, likes to refer to newly discovered relationships in nature as results "which would have pleased old Hegel." His point of view is, however, not that of Hegel's idealism, but of a consistent materialism. The dialectical relation between the different categories in nature is not for Engels a relation in the human mind only or better, in Hegel's language, in the "Absolute", but is an image of the relation existing in nature itself independently of the human mind. In this way we have in the "Dialektik der Natur" an important contribution to dialectical materialism, of which Marx and Engels were the discoverers.

DIRK JAN STRUIK.

The Natural Sciences, by Bernhard Bavink. Translated from the 4th German Edition, with additional notes and Bibliography for English Readers, by H. Stafford Hatfield. 683 pp. New York: The Century Company. 1932.

The availability in English of Bavink's *Ergebnisse und Probleme der Naturwissenschaft*, which has already passed through four German editions, marks an event of some importance for those who are interested in the development of the philosophy of science in this country. For such a book as this with its well-defined problems and its cautious method serves to mark out a path in a field which is unexcelled in the opportunities which it offers of being led astray. In the philosophy of science one must avoid not only the good scientists who write bad philosophy but also the good philosophers who are ignorant of science. In Bavink we have the remarkable combination of a complete familiarity

with the conclusions and methods of science and a sound and sober philosophical judgment. In addition to this we find a lucidity of style which makes his book a delight to read. For these reasons the book is one which no one who is seriously interested in the subject can afford to neglect.

The book is divided into four parts of unequal length entitled, respectively, Force and Matter, Cosmos and Earth, Matter and Life, and Nature and Man. One wishes that the author might have extended his subject to include a division concerned with the mathematical sciences; this would have given the discussion a certain completeness. As it is one is led to wonder occasionally just what position mathematical entities occupy in the general scheme of things, particularly when he attempts to draw conclusions as to the psychical character of existence upon the basis of the timeless nature of the laws of pure arithmetic (533). As is to be expected, the discussion is concerned mainly with scientific subject-matter. But the philosophical reflections are not confined to random comments; on the contrary there seems to be a conscious aim to develop wherever it is possible a unified philosophical outlook. One notes time and again a certain feeling of reluctance on the part of the author to dismiss a scientific question without some philosophical remark, if it is nothing more than the assertion of the impossibility of concluding anything of philosophical significance from the scientific fact. Yet in all of the philosophy there is the most conspicuous lack of dogmatism. In almost no cases are we permitted to infer a philosophical conclusion with anything more than a slight preponderance of evidence; in the great majority of cases the author asserts quite definitely the impossibility of concluding *anything* of a philosophical character from the specific evidence revealed by science. Thus one feels that considering the author's urgent desire to express himself philosophically and yet in view of his reluctance to assert unfounded philosophical propositions, we have here within the compass of some six hundred pages all, perhaps, that one can legitimately conclude as to the character of the world as a whole upon the basis of the revelations of science.

In view of the size of the book and the range of topics considered, I have chosen in what follows to select from and to neglect the topical order of the table of contents. I shall therefore discuss the author's treatment of his subject under the five general headings: The Status of Scientific Objects; Causality; Mechanism *vs.* Vitalism; The Spiritualistic World View; and Value.

In considering the question as to the metaphysical status of scientific objects, the author leaves no doubt in one's mind either as to his own anti-positivistic position or as to the meaning and inadequacies of the positivistic answer. If one makes a distinction between two kinds of theories, he asserts, the nature of the latter solution to the problem becomes quite clear. On the one hand there are elaborative theories whose characteristic is such that they contain practically no hypothetical elements, the fundamental assumptions in them being, as in the case of Fourier's law of heat conduction and in the laws of the reflection and refraction of light, essentially the data of experience. On the other hand there are those theories which may be called explanatory, of which the atomic theory is the best example. The characteristic of this kind of theory is that the desired logical connection and unification of the facts is reached only by a speculative assumption which is described as the hypothesis upon which the theory is based. Such theories contain one or several elements which are completely outside experience as so far known. The fundamental claim of positivism (Mach, Ostwald, Pearson, Rankine) is that only the former type of theory has a legitimate place in science. The latter type can be included in science only if one is able to differentiate what is tenable and useful in them from the speculative and hypothetical elements. Hence the latter type are useful only in describing and simplifying phenomena, and they lead to error as soon as one supposes that they bring us nearer to the nature of things.

While himself rejecting the positivistic position, the author is quite ready to admit its value. Positivism is right in insisting that a supposition is not the same thing as a discovered fact. Furthermore it is right in pointing out the danger in attributing to the fact the pictorial accessories of the theory. But on the other hand positivism is definitely wrong in excluding from science all types of theories which contain pictorial elements, because the history of science has shown us that provisional constructions turn out, on many occasions, to have indisputable reality. "A physical hypothesis is the presumption of the existence of a general state of affairs, lying at the back of certain phenomena which are matters of experience, and allowing the phenomena in the field of facts in question to be deduced qualitatively and quantitatively (mathematically) from the presence of the said state of affairs and its assumed laws. . . . The indirect confirmation of an hypothesis obtained in this way may be so convincing that its probability practically amounts to certainty (42)." For the author himself "the real existence of atoms and molecules is an undoubted fact (19)." No one asks any

longer: in what respect do things behave 'as if' they consisted of atoms? Atoms have the same status as cells.

In discussing the problem of causality Bavink begins by analyzing a number of types of causal judgment. From this analysis he concludes that "a theory of causality must of necessity fulfill these two main conditions, firstly that of necessity and secondly that of direction (69)." But this directional character is not to be identified with time. It is true that in the majority of cases the laws are of the kind which connect the momentary values of certain magnitudes with their changes with respect to time, as in the case of mechanics. This has led to the error of including time in the form of the causal relationship instead of in its content. The directional character of the law is found in the logical relation by which the whole complex of circumstances which we describe as the cause is the 'prius' of which the effect is the conclusion or logical 'posterius.' Furthermore these two characteristics of a causal law may well be satisfied by a statistical law. "When we have perceived that the increase in entropy (equivalent to a decrease of free energy) happens because there is a transition from a less probable to an enormously more probable condition, this is just as good a satisfaction of our logical instincts as when we deduce on strictly causal lines the form of the planetary orbits by means of Newton's law of gravitation. In both cases, our question as to the reason is answered as far as such an answer is possible within the frame of a definite small region of world events (213)."

The problem of mechanism *vs* vitalism is one which science is incapable of deciding unless further arguments on one side or the other can be produced. Though mechanism has hitherto made several victorious advances against vitalism as regards the phenomena of life, the prospects of mechanism as regards the origin of life become less and less the deeper becomes our insight into cell structure. It does seem possible to conclude quite definitely that it is hopeless for the vitalist to base his position on the demonstration of certain processes or groups of processes which are supposed not to occur in inorganic systems. Every assertion of this kind can be refuted by the mechanist in terms of some striking analogy in the inorganic realm. On the other hand, vitalism is not inexact; "an 'entelechy' need not be in itself any more mysterious than a physical 'force' (386)." Nor can the principle of causality be given as a decisive reason for mechanism against teleology, for science is any logical connection of facts in thought, whether it be etiological or teleological. Science, however, does suggest a new direction in which one

may look for a solution of the conflict. Might we not attempt to reconcile the opposing views in the same way that modern physical science is attempting to avoid the contradiction between waves and corpuscles, i.e. by resolving them into a higher unity . . . at the expense, however, of an almost complete loss of physical comprehensibility? Similarly might it not be shown that physical causality and conscious free action of man are only the extreme ends of a series? Though they appear to us as opposites perhaps they are only contraries, thus there might be a more general type of action of which they are extreme differentiations.

Is it possible, then, upon the basis of the data revealed by science, to draw any conclusions as to the general character of the world as a whole? In metaphysics we seek "nothing more than a general view of the world and the relationship of its various types of phenomena (body and soul, nature and civilization, etc.) to one another, which on the one hand allows of direct connection with the results of the separate sciences, but on the other hand also brings this theoretical knowledge of ours into rational connection with the other factors of our cultural life (427)." Such a world view will probably be of a spiritual character, at least it cannot be materialism. I cannot enumerate all of the considerations which the author brings up in support of this position. But a few may be mentioned. In the first place the continuity of the higher forms of life with the lower forms argues for the existence of spiritual processes in all life. But, in the second place, the individual is not an absolute unit which can be limited spatially or temporally; it is impossible to say precisely at what point the individual begins life, or at what point life ends. Or when the severed earth worm develops two complete worms from the segments, at what point did half a worm become a whole worm? This permits us to argue that the individual is perhaps a wave, more or less well defined, on the great stream of organic happenings for which it is not possible to give either a beginning on an end point. In the third place, "from the atom to the world of fixed stars, from amoeba to humanity, there is an almost uninterrupted series of steps in the formation of ever higher and more comprehensive wholes (433)." Again, "it is not improbable that physics will be able to eliminate the concept of substance from its system completely before very long, all its laws being then reduced to formal, arithmetical relations of an indifferent Something. . . . This something might according to its nature be psychical. . . . The whole material world would, therefore, be only a formal constitution of this psychical, in which it appears for one of its own parts, namely the individual soul (532, 3)." Once more, "who can guarantee

that the spiritual does not reach out beyond the limits of the individual? We know nothing in detail about its connexion with the bodily, we know only that in Minkowski's language it is peculiar to certain 'world-line' complexes, that these serve as 'guides' or whatever we like to call them for the spiritual (this is certain as regards the physical processes taking place in our brains). Who is to tell us where this possibility ceases? (424)." Finally and most conclusively there are all of the facts of the unconscious, hypnosis, telepathy, clairvoyance, and paraphysical phenomena. It is clearly impossible to explain these phenomena upon any grounds. But upon the spiritual hypothesis they may be understood in a general way. If the unquestionable universal inter-connexion of nature has its counterpart in a similar connexion in the spiritual world, it might be said that "the upper or self-consciousness is like the fairly narrow, brightly illuminated, surface of the sea upon which the light from a lighthouse falls, while the unconscious is the dark and unfathomable depths which lie below it. . . . Telepathy would then be regarded as a faculty possessed by certain people who are able, more easily than the vast majority, to descend into the depths of the unconscious (530, 1)." By means of this activity they are able to get into touch with spatially distinct individuals, just as through clairvoyance (prophecy) they are able to get into contact with temporally distinct situations.

The outcome of such considerations as these is a probability in favor of a panpsychistic world, a world consisting of a "hierarchy of objectivised ideas (434)."

Finally, what light has science to throw upon the problem of value? In our general value outlook we are compelled to recognize five values: the True, the Good, the Beautiful, the Useful (*Zweckmässig*) and the Holy. What can science say as to the status of these values? Just as the whole of science would be meaningless without the belief that the striving after truth is not in vain, so we may by a justifiable analogy carry over this conclusion to the fields of the other values. Thus we have a right to believe in the objective existence of the Good, the Beautiful, etc. "This means the return from individualism to an objective point of view, from absolute idealism to realism, but to critical realism . . . which . . . remains aware that it is the very characteristic of objective truth to transcend all purely human measure (592, 3)." Unfortunately, however, the question as to why the world happens to have value is just as unanswerable as the question as to why the world exists. There is an ultimate indetermination either in the 'historical prime structure' or in the 'theoretical prime structure' or in both. It is

therefore just as hopeless to try to find the final values as to find the final facts. But where science may fail art may succeed; thus it may be that the key to reality lies not in scientific formulae but in artistic inspiration. For often in music "you get a faint inkling of the reason why God did not remain God alone, but created a world with joy, life and love, but also with pain, death and sin, and of where the solution of this contradiction is to be found. No philosopher in the world can tell you more about it than Bach, Beethoven, and Brahms (610)."

Space does not permit criticism—a fact which causes me no regrets. Suffice it to say that were I to criticize, I should direct my attacks against his epistemological rather than against his metaphysical conclusions. Since the latter make up the larger part of his position, what I should have to say would be directed against certain random reflections and not against the general thesis of the book, if there is any such thesis. At any rate where one's conclusions are only of a probable character and there is no way of measuring the degree of probability it would be futile to argue. The fact remains that the author's remarks are everywhere penetrating and clear-cut even when not plausible.

A. Cornelius Benjamin.

A NEW BUDGET OF PARADOXES

"A New Budget of Paradoxes" will be a section of our Journal devoted to the pathological. It is not mere amusement. The old "Budget of Paradoxes," edited by Augustus De Morgan is our model both for choice of material and for humaneness toward the Don Quixotes of Thought.

(1) THE NEW PARADOXER

Says De Morgan: "A great many individuals, ever since the rise of the mathematical method, have, each for himself, attacked its direct and indirect consequences . . . I shall call each of these persons a *paradoxer*, and his system a paradox." It seems that potent blessings—or what not—have had their effect, for the tribe has increased. Possibly we have learned to spot more varieties. At any rate the De Morgan varieties have given way to new ones. Circle-squaring is not entirely out of fashion; nor are attacks on Newton, fantastical demonstrations that the Bible is the last word on science, and the like quite gone. Perhaps the WANDERING PARADOXER has but changed his clothes and is still the same in his perpetual youth. He presents himself now as one kind now as another. The writer has stalked him for years, and submits the following identifications:

Class I. Emulsion-Makers
- A. Specialists in Non-sequiturs, thumb logic
- B. Eclectic Kleptomaniacs

Class II. Phantom-Criers
- A. Creators of Straw Men
- B. Alarmists

Class III. Magicians
- A. Paladins of the Balance
- B. Illusionists

Class IV. Poisoners
- A. Annihilators (Borgiac, Neronic, etc.)
- B. Glossifiers

Class V. Overmen
- A. Gods
- B. Devils

We invite clinical contributions retailing observations on choice cases.

W. M. M.

NOTICES

Papers soon to appear include the following:

Meaning and Scientific Status of Causality, by H. Margenau.
Time, Space and Gestalt, by O. Reiser.
The Mystery of Scientific Discovery, by A. C. Benjamin.
On the Construction of Functional Classes in a Mechanical Medium, by E. A. Singer, Jr.
What is Analysis? by A. E. Blumberg.
An Examination of the Quantum Theories. II, by W. M. Malisoff.
The Psycho-Physical Problem, by H. Feigl.
The Foundations of Mathematical Bio-physics, by N. Rashevsky.
Critique of Freud, by J. F. Brown.
Philosophy of Science in America, by M. R. Cohen.
What is the Case? by L. S. Stebbing.
Scales and Measurement, by W. Bender.

All contributions are examined by sub-committees of the editors and members of the advisory board. In general unanimous consent is required to ensure publication.